EVOLUTION OF SOUND:

A PART OF

The Problem of HUMAN LIFE Here and Hereafter

CONTAINING A REVIEW OF

TYNDALL, HELMHOLTZ, AND MAYER.

BY WILFORD.

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PREFACE.

If one object more than any other has exercised a controlling influence over my thoughts and motives in the preparation of this volume, it has been to throw, if possible, some new light from a philosophical and scientific standpoint upon the problem of man's conscious and substantial existence beyond the present life.

Aware of the almost numberless books which have appeared from time to time during the last hundred years with this object partially or wholly in view, I still could not help feeling that the subject had not yet become exhausted. The impression seemed to fasten itself upon me that whether or not I should succeed in finding a single grain of additional golden truth, there nevertheless remained hidden beneath the scoria and rubble of the scientific investigations which are now agitating the minds of advanced thinkers, undreamt-of lodes of precious evidence, favoring, if not absolutely demonstrating, a future state of being,—while in no department of philosophical or biological research were such stores of evidence likely to be discovered so richly deposited as in that which includes the great and complicated problems raised by Modern Evolution.

It is a fact which thoughtful minds can not fail to recognize, that no philosophical theory in any way related to man's origin or destiny, or which in any degree involves man as a sentient and intellectual being, has ever so suddenly sprung into popular favor or taken such general possession of all classes of scientific thinkers as this modern crusade against religion popularly known as Darwinism.

I therefore felt, after years of reading and thoughtful study and after carefully considering the true basis on which this theory rests, that no line of philosophical, metaphysical, or physiological discussion, could possibly furnish so varied an opportunity as this for directly and indirectly unfolding any new ideas I might have hit upon during my investigations bearing on this question of all questions—Are we destined to live after this earthly pilgrimage is ended, or is conscious existence eternally blotted out at death?

Whatever scientific or philosophical discussions, therefore, may be found incidentally woven into this book, they will prove to have an indirect if not a direct bearing on this unparalleled problem of man's perpetual existence. Many of the subjects introduced and much of the reasoning concerning them will no doubt at first strike the reader as irrelevant to this central and paramount question of a future life; yet still, if the arguments are followed out to their legitimate aim and culmination, they will be seen to tend toward the predominant thought that all things in Nature which exist or can form the basis of a concept are really substantial entities, whether they are the so-called principles or forces of Nature or the atoms of corporeal bodies, even extending to the *life* and *mental powers* of every sentient organism, from the highest to the lowest. And since science has determined that no substance in the universe can be annihilated, there must therefore be deduced a scientific basis for the immortality of the soul if the life and mind should be conclusively shown to be substantial entities.

It matters not, therefore, what analogical questions or facts of science may come before the reader in the preliminary chapters of this book, such as those relating to the substantial or entitative nature of Sound, Light, Heat, Gravitation, Electricity, Magnetism, Odor, Air, &c., they have one intrinsic and paramount object constantly in view, and that is, to insensibly but surely prepare the way for an intelligent conviction in the mind of the reader that the present life can not, in the very nature and fitness of things, be all there is of us or for us.

In view of this matchless consummation, I now venture the assertion that the reader will find, ere he finishes this volume, numerous scientific proofs which may be fairly classed as demonstrative, showing that the life and mental powers are as really substantial entities, though intangible to the physical senses, as are the blood, bone, and muscle, constituting our corporeal organisms.

A writer in the *North American Review* (Thomas Hitchcock), after showing the entire reasonableness of the substantial nature of the soul, calls upon scientists for the physiological and psychological facts which shall demonstrate it, and truly adds: "Certainly, the achievements of science, of which we boast so much, are worth but little if they can not aid us to solve this problem." The facts thus called for are to be found in this volume, though they were written and in type months before the article referred to appeared in the *Review.*

For many years I have had incessantly before me, as the crowning ambition and culminating triumph of my earthly existence, this one superlative achievement, namely, to add a few rationally scientific reasons, hitherto undiscovered, which should go to render a future conscious state of being for man clearly probable, aside from and in addition to theological considerations, and thus bring the certitude of immortality so far into accord with the settled principles of philosophy and science—making it so harmonious and consistent with the current modes of thought—as to command the attention and respect of advanced thinkers and investigators in whatever department of scientific research.

To accomplish so grand a work as this, I saw plainly that, first of all, the complete overthrow of evolution, by the destruction of the main arguments on which it rests, had become an absolute necessity; for so long as naturalists can triumphantly point to one of their leading scientific facts or physiological phenomena which has not been fairly wrenched from the grasp of evolution, so long will all scientific evidence of man's intrinsic susceptibility of and primordial adaptivity to an immortal state of being have with them but the weight of a provisional hypothesis.

Prior, however, to undertaking the task of breaking through the entrenched works of the evolutionist, and in order to prepare the reader for placing the proper estimate upon these so-called scientific theories which assume to overthrow religion,—such, for example, as Mr. Darwin's doctrine of man's development from the monkey,—I resolved, as an example of what might be expected in the future, to attempt the overthrow of one of the universally accepted theories of science,—a theory which has never been called in question by any writer on the subject, and one which is considered to-day by all scientists as firmly established as the Copernican Theory of Astronomy, or as little to be doubted as the law of gravitation, namely, the Wave-Theory of Sound, out of which has been developed the Undulatory Theory of Light and the more recently constructed theory of Heat as a Mode of Motion.

In this seemingly preposterous and hazardous attempt I was necessarily compelled to undertake the additional task of reviewing no less an authority than Professor Tyndall (the ablest and most popular exponent of the sound-theory now living), and of thus demonstrating the complete unreliability and defenselessness of the scientific opinions and statements of one of the most aggressive advocates of modern evolution, even when treating on the simplest facts of science and making the most ordinary philosophical deductions.

If I have succeeded in this attempt, and if the wave-theory of sound has had to succumb fairly to the arguments brought against it, in defiance of the supposed facts and demonstrations published to the world by this highest living authority, then the reader may justly discount evolution in advance as having no sort of claim on the belief of mankind based on the ground of scientific authority.

I had, moreover, another and distinct object in view in attempting to break down and revolutionize the current sound-theory, as the reader will frequently observe coming to the surface, and that was this: If the wave-theory of sound is really a fallacy in science, then nothing remains to be accepted but the hypothesis that sound consists of corpuscular emissions and is therefore a substantial entity, as much so as is air or odor; and if sound is thus absolutely proved to be a substance, *there can not be the shadow of a scientific objection raised against the substantial or entitative nature of life and the mental powers.*

In that portion of this work relating directly to the review of Mr. Darwin's theory of transmutation, I have sought primarily to present the arguments in opposition to evolution, spontaneous generation, &c., in such concise and simple language as to make every question discussed at once understood by the most ordinary reader. In seeking to avoid circumlocution, I may have sometimes gone to the extreme the other way; and in aiming at directness of results by dealing with and massing solid and naked facts, may have occasionally hurled too abruptly the monstrous inconsistencies of the doctrine into the teeth of evolution. Whatever apparent want of courtesy certain passages may have at times betrayed, nothing but the kindest of feelings and highest personal and professional regard for the great authors I have had occasion to review, coupled with an earnest desire to rivet the truth and force of my arguments upon the memory of the reader, has had the slightest influence in dictating the tone of such occasional paragraphs.

I have therefore made it my leading object to conduct the discussion and condense the arguments against the theory of man's descent by transmutation from lower animals in such a manner that the most superficial reader shall hereafter have the weapons at hand to meet with irresistible effect even the acknowledged champions of the system, if need be, and thus put a check to its progress where most required.

With what success the following pages shall have carried out this programme, and to what extent they may in the future accomplish the result intimated, the reader must judge after he has perused the volume. It need only be added that the work is frankly offered to the public as an imperfect and humble contribution to what is believed to be the cause of true scientific knowledge, by

THE AUTHOR.

NEW YORK, June 1, 1877.

PREFACE TO THE SECOND EDITION.

In this edition Chapters V. and VI., on *The Nature of Sound*, have been wholly re-written. The investigation was of such a revolutionary character, and involved so many questions of science considered thoroughly established, that it was found impossible for the writer to properly discuss the old theory of sound, or present the claims of the new hypothesis, without further consideration than he was able to give the subject when first preparing the work. He also found that, in the hurry, he had committed a few errors which were necessary to be corrected, and had written some things which were deemed advisable to be left out of the work.

The *Evolution of Sound*, as thus revised, is now dedicated to the scientific investigators of Europe and America, with the kindest wishes of

NEW YORK, June 1, 1878. THE AUTHOR.

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The Origin of Wings in Birds, Bats, and Insects, Wholly Inexplicable on the Principles of Natural Selection.—A Difficulty which Evolutionists never Attempt to Meet.—Natural Selection can Only Work on Useful Organs and Variations.—Incipient Wings shown to be not only Useless but Injurious, if they ever Existed.—As Natural Selection can make no "Leaps," Wings must have been Miraculously Created.—Reasons for this Conclusion.—The First Wings demonstrated to have been Miraculously Formed.—All Mechanical Operations which Overcome Laws of Nature, Supernatural.—No Device, such as a Wing, where Multiplied Parts show Design for One End, can Result without Primordial Intellect.—The Flying of Human Beings, by Mechanical Wings Alone, not only Possible but Probable in the Near Future.—Mr. Darwin's Theory Again Breaks Down by his own Express Stipulation.—The Rattlesnake's Musical Appendage could not have been Started by Evolution, even if it could Afterward be Improved by it.—The Venom of Serpents Conclusive Proof Against the Theory, being a Wonderful Chemical Combination Relating Solely to Other Organisms.—It could only have Originated by Prior Intelligence.—The Vegetable Kingdom has Many Examples of Design, and a Clearly Intelligent and Preconceived Intention.—The Pappus of the Thistle and Dandelion, for Carrying Seeds through the Air, could not have Originated by Natural Selection, as their Incipiency would have been Wholly Useless.—Mr. Darwin Admits that on Certain Conditions his Theory would be Annihilated.—The Conditions Distinctly Complied With, to the Letter.—Peculiar Odor and Flavor of Ants and Bees made for the Special Benefit of Other Species.—The Odor of the Fox's Feet not for its own Good (since it leads to its Destruction), but for the Advantage of the Dog and Wolf.—Inconsistencies of Evolution Pointed Out.—The Mane of the Lion claimed by Mr. Darwin to have been Developed as a Protection.—The Question of the Neck of the Giraffe having been Elongated to Reach the Branches of Trees Examined.—The Whole Supposition Shown to be Clearly Absurd.—The Trunks of Elephants Considered.—The Hive-Bee's Sting Developed to Cause Suicide if Used.—Natural Selection could not have Produced it.—Useless Bees, such as Hornets, Wasps, and Bumble-Bees, can Sting Without Danger to Themselves.—The Reason Why, and a Design in this Difference.—The Mimicry of Insects, Worms, &c., for Protection from Birds, Examined.—Mr. Darwin Congratulates Himself that he has Aided in Overthrowing Creation.—A Former Pledge Redeemed.—Professor Haeckel Proved to have Unwittingly Yielded the Whole Question of Evolution.—He is Indorsed by Mr. Darwin.—The Proof Conclusive.—Mr. Darwin again Admits his Theory will "Break Down" on Certain Conditions.—These Conditions Pointed Out in Hundreds of Instances.—He Furnishes Himself the Direct Proof which Breaks Down his Theory.—He Virtually but Unwittingly Admits that Wings must have been Created.—Self-Contradictions and Inconsistencies Multiply.—The Theory of Descent Hopelessly Breaks Down.

CHAPTER V.

EVOLUTION OF SOUND.—REVIEW OF PROFS. TYNDALL, HELMHOLTZ, AND MAYER.

The Wave-Theory of Sound Assailed.—A New Hypothesis of Substantial Sonorous Corpuscles Proposed.—The Difference between the two Hypotheses Pointed Out.—No Middle Ground is Possible between the two.—Hence, if Wave-Motion Breaks Down the Corpuscular Hypothesis must be Admitted.—All Phenomena of Sound claimed by the Writer to be Explicable on the basis of Substantial Pulses.—Several Illustrations Given.—Sympathetic Vibration Explained.—Resonance Proved to be Utterly Inexplicable by the Wave-Theory.—Many Illustrations brought to bear.—The Superficiality of Physicists Pointed Out.—Laughable Illustrations from Tyndall and Helmholtz.—Resonance Explained.—The True Law of Sound-Generation given for the first time.—Magazine Explosions Considered, and Turned Against the Wave-Theory.—Professor Mayer's Unphilosophical Reasoning Reviewed.—The Falling Pitch of a Locomotive-Whistle on Passing a Station Considered.—Other Objections Answered.—Reflection and Convergence of Sound Explained.—"Condensations and Rarefactions" shown to be Fatal to the Wave-Theory.—The Illustration of the Stridulation of a Locust shown to be Disastrous to the Wave-Hypothesis in many ways.—Professor Mayer's Fatal Admissions.—A Locust must exert Millions of Tons of Mechanical Force by the Motion of its Legs if the Wave-Theory is true.—Shown in Numerous Ways.—A Serious Scientific Mistake Perpetrated by Professor Tyndall.—The Propagation of Sound by Means of Sonorous Corpuscles Explained and Contrasted with Wave-Motion.—The Discrepancy Discovered by Newton of 174 feet a Second in Sound-Velocity Fatal to the Theory.—Laplace's Solution Proved Fallacious.—The Law of Sound-Velocity, or the Relation of Density to Elasticity, Examined.—Amusing Self-Contradictions of Professor Tyndall.—Why has the Current Theory of Sound, if False, not been Assailed before?—An Overwhelming Argument against the Theory drawn from the Supposition of Tympanic Vibration.—Over-Tones, Resultant Tones, &c., Examined.—Helmholtz's Analysis of the Ear Reviewed.—His Numerous Self-Contradictions and Inconsistencies Pointed Out.—Beautiful Analogies in Nature favorable to the Corpuscular Hypothesis.

Up to this point in the investigation of the so-called natural forces or modes of motion, I have only hinted that Sound, as well as Light and Heat, must, in the very nature and fitness of things, be a substantial entity, consisting of corpuscular emissions or some kind of atomic emanations. I now come to the work of argument and proof, and shall endeavor to satisfy the reader, in this and the following chapter, however exacting he may be, not only that the above position is every way reasonable and probably true, from innumerable facts and analogies, but that the current and universally accepted wave-theory of sound is demonstrably a pure and simple fallacy of science, founded upon the most superficial misapprehensions of Nature and her laws,—thus rendering the substantial nature of sound logically sustained by excluding the only other possible assumption—wave-motion.

I am aware of the magnitude of the task I have undertaken to perform, and have considered well the full import and consequences of assuming in this seventh decade of the nineteenth century to overturn an established theory of science,—especially a theory like that of *Sound*, which has not only stood unshaken for centuries, but has never been so much as called in question or doubted by a single scientific writer for 2,500 years, or since its origination in the time of Pythagoras.

The truth is, the wave-theory—or, as it is popularly known, the undulatory theory—of sound has been so long in existence with no one to question its correctness, that modern physicists have been in the habit of accepting it, handed down from generation to generation, with all its unspeakable difficulties, as a kind of legacy bequeathed from scientists of the past; and, with an acquiescence unparalleled in the annals of physical investigations, have labored to explain its inexplicable contradictions and reconcile its infinite absurdities, with a patient persistence which a love of science can alone inspire. Hence it is that no physicist has had the hardihood, if he had the originality, to cut loose from the ancient landmarks of the theory, or to venture an hypothesis to take its place. The writer of these chapters is a solitary—possibly an unfortunate—exception, the result of whose venture the following pages will disclose.

I will only extend these introductory remarks here by adding that I have not ignored the important fact in thus attempting to revolutionize the theory of Sound, that I have to meet face to face the powerful intellectual abilities of such physicists as Helmholtz, Tyndall, Kuntz, Blacerna, Mayer, and a host of others, either one of whom, when it comes to the investigation of questions relating to physical science, is sufficient to make a cautious writer quail and hesitate, and even repudiate the deliberately formed convictions of his own judgment. This was the actual impression on my own mind for many months before putting pen to paper, even after I had become thoroughly satisfied in reading, experimenting, and investigating, that the wave-theory though ingenious was purely visionary, having not a single correctly understood fact of science on which to rest. I have at last thrown off my natural timidity and hesitancy, and, though the combat may be mortal on my side, I shall not have proved the first one who has immolated himself upon the altar of his scientific convictions.

While discussing the question of light in the preceding chapter, and examining the modern undulatory theory as a substitute for Sir Isaac Newton's corpuscular theory, I took occasion to point out the fact, that, had Newton taken advantage of the new feature of this hypothesis, namely, that light itself, as a substantial corpuscular emission, was radiated from the light-producing body in *pulses* or luminous *discharges*, he need never have been driven from his ground of light as substance, and been forced to admit certain phenomena which could only be explained by wave-motion; for, according to this view, now for the first time publicly presented, that light is generated by the incandescent tremor of the luminous body and diffused through space in luminous *pulses* or *discharges* which synchronize with such tremors, there is no use whatever for a substantial luminiferous ether (by the way, a pure invention gotten up to meet this very case), since the pulses or discharges of light-corpuscles themselves would have answered the same purpose as ether-waves, and would have thus solved every problem which could have been possibly explained by the latter hypothesis.

Sound is a parallel phenomenon every way we can view it, as it is well known to every scientific student that it was only the universally acknowledged fact that sound-phenomena resulted from the supposed undulatory motion of the air, which led philosophers to the invention of this all-pervading luminiferous ether, extending, as is supposed, to the very outmost limits of telescopic vision, if not throughout all space. When Professor Young first suggested such a substance as *ether*, whose undulations might explain certain phenomena resembling those of sound, which no one had ever suspected to be other than caused by air-waves, it did not occur to this learned investigator that air-waves themselves, as the means of sound-propagation, were a pure fallacy of science, without one fact, or, when fully analyzed, appearance of fact, to warrant them,—as will fully appear in due time.

I am well aware that an intimation like this, after so many learned treatises on sound as the result of wave-motion have appeared from pens like those of Helmholtz and Tyndall, will naturally awaken in the scientific mind a feeling of contempt for its author, mingled perhaps with commiseration. Even my most intimate friends have warned me to desist from publishing these chapters, unless I wish to make myself ridiculous in the eyes of the scientific world, and be set down as a first-class candidate for a lunatic asylum. But as I have counted the cost and am not at all convinced of my insanity, I have, of course, declined the advice so gratuitously tendered.

Before introducing a single argument against the hypothesis that sound is propagated by means of atmospheric undulations or any other kind of wave-motion, I wish to clearly state the difference between the old and the new hypothesis of sound-propagation, and to name some of the well-recognized facts of these phenomena, on which there can be no controversy or difference of opinion, as the basis of all future argument. I do not propose to tear down the wave-theory without framing an hypothesis to take its place, and one which will serve as a basis for the solution of the undeniable problems presented in sound-phenomena. While maintaining, as I do, that the wave-theory is a most transparent and unmitigated scientific fallacy, I as strongly insist that, such fact being clearly established, there is nothing else left for sound to be but *substantial emissions*. It does not seem to me that a reflecting mind can draw any other conclusion than corpuscular emanations of some kind of substance, however attenuated it may be, if first of all the wave-theory breaks down hopelessly, as I shall attempt to show it must.

Even if the substance constituting these sonorous pulses were conceded to be as attenuated as the material atoms composing Professor Tyndall's *gelatinous luminiferous ether* which forms the basis of light-waves, I should still maintain that such substantial emanations are every way reasonable and consistent with Nature's analogues, many of which I will take occasion to introduce as the argument advances, while no advocate of the undulatory theory of light, and of these substantial *waves* of *ether* moving freely among the molecules of the diamond, can reasonably object to substantial discharges of sound, when, as I have shown in the preceding chapter, light itself could just as well be supposed to radiate in the form of substantial waves or pulses, as first to ignore such a substance entirely, and then substitute another material (luminiferous ether) almost infinitely more difficult to accept.*

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* "To account for the enormous velocity of propagation in the case of light, *the substance which transmits it is assumed to be of both extreme elasticity and extreme tenuity*. This substance is called the *Luminiferous Ether*. It fills all space; it surrounds the atoms of bodies. . . . The molecules of luminous bodies are in a state of vibration. The *vibrations*

I admit at once, in thus assuming what must now be unavoidable in my hypothesis,—namely, that the chirping of a cricket fills the surrounding air with substantial emanations,—that I invite, at first sight, the incredulity if not the ridicule of all scientific thinkers; but while this hypothesis will be shown to be entirely consistent with other well-known natural phenomena all around us, which no well-informed mind can doubt, it will be demonstrated that, according to the universally accepted wave-theory, the cricket is actually made to perform a miracle of physical power compared to which the crushing of a granite rock to powder by the drifting against it of a thistle-pappus would be as nothing.

I may also add, in this connection, that it never was thought of being urged in the arguments with Sir Isaac Newton, who strongly held to the corpuscular theory of light, that there was any possible middle ground between that view and the undulatory hypothesis; but rather it was tacitly conceded that if one was disproved the other was clearly substantiated. It was never intimated by any opponent of Newton's hypothesis—not even by the great mathematician Laplace—that if ether-waves were absolutely shown to be fallacious and impossible, some other hypothesis might be suggested besides substantial emanations. It seemed to be conceded on all hands that if wave-motion fell to the ground, the fact became established that light as substance of some kind must be taken for granted.

are *taken up by the ether* and *transmitted through it in waves*," &c.

"In fact, the mechanical properties of the *ether* are rather those of a *solid* than of an *air*."—"The luminiferous *ether* has definite mechanical properties. It is almost *infinitely more attenuated than any known gas*, but its properties are those of a *solid* rather than those of a *gas*. It resembles *jelly* rather than *air*."—TYNDALL on "Light," pp. 57, 60.

So, also, stands the question as regards sound. If atmospheric wave-motion is ruled out by fair logic and incontrovertible facts, there is no middle ground which can be assumed between it and substantial emissions. Professor Helmholtz lays down the principle in logic and science that a proposition is fairly sustained by the exclusion of all other supposable assumptions. I shall therefore avail myself of this logic (since something must cause the sensation we term sound), and insist that if I shall clearly succeed in demonstrating the fallacy of wave-motion as the cause of sonorous sensations, then the corpuscular theory becomes necessarily established till such time as physicists shall discover and elucidate some more plausible middle ground as a solution of sound phenomena. I doubt not the scientific reader will readily admit the fairness and logical necessity of the position here assumed.

What, then, is the real difference between the two hypotheses, one or the other of which must be accepted?

Sound is undoubtedly generated by the *vibratory motion* of whatever instrument produces it, just as light is admitted to have its origin in the *tremulous motion* of the incandescent molecules of luminous bodies. Sound thus produced is claimed in this hypothesis to be a finely attenuated substance, which is radiated from the sound-producing body by an unknown law of diffusion, just as the radiant atoms of light, heat, magnetism, electricity, and even odor, are sent off from their respective sources.

Science, as yet, has given us no light on the subject of radiation or conduction. It even can not explain osmotic action, or why liquids of different densities tend to mix or project their particles through each other, in opposition to the law of gravity; or why grains of odor tend to shoot through still atmosphere at considerable velocity, much less by what law magnetic atoms dart off from the poles of a magnet in ceaseless streams, or what motile force sends electric fluid through a wire at almost inconceivable velocity. It is enough for us, in the present investigation, to know that such laws of radiation and conduction do exist, and that each of these incorporeal substances named, if they be substances, such as Light, Heat, Magnetism, Electricity, Gravitation, Odor, and Sound, has its own peculiar law of radiation and conduction, suited by the Allwise Lawgiver to the use which each of these imponderable substances is intended to serve.

As sound is generated by the vibratory action of the instrument which produces it, and consists (as I assume) of atomic emissions, it is in strict accordance with philosophy and reason that these corpuscular emissions should be radiated in sonorous *pulses* or *discharges*, instead of continuous streams, each discharge synchronizing with the vibratory movement of the string or other instrument which generates it, exactly as I have assumed light to be emitted from stellar bodies.

The distance between these discharges as they pass off, or the interval occurring between their transmissions, determines the pitch of the sound. If the vibratory oscillations of the instrument be slow, thereby causing a low pitch, then the synchronous discharges of the sonorous substance will strike the tympanic membrane of the distant listener exactly the same intervals apart, and consequently will produce the same pitch of tone there. But if the sound-producing instrument vibrates rapidly, the sonorous discharges must necessarily pass off with a corresponding rapidity, and reach the ear with a correspondingly higher pitch of tone. Such discharges radiate through the atmosphere at ordinary temperature—say sixty degrees Fahrenheit—at 1120 feet a second, as proved by careful observation.

If sound consists of substantial atoms, as I propose to show must be the case before I conclude this treatise, then it must travel through whatever body conducts it—let that be air, water, wood, or iron,—in the manner here described, namely, as sonorous *pulses* or *discharges*, such discharges and vibrations keeping up their perfect synchronism or periodicity.

The current theory of sound, in speaking of these sonorous discharges, calls them "*air-waves*," and the intervals occurring between them "*wave-lengths*," which determine, in the same manner as I have described, the pitch of tone. If the vibratory motions of the instrument be *slow*, the *air-waves* supposed to be "moulded" and sent off by such vibrations are said to be *long*, or to be of a considerable distance from crest to crest or from sinus to sinus, or, to use the technical phrase, "from *condensation* to *condensation*, and from *rarefaction* to *rarefaction*," as expressed by all writers on the subject. If the vibrations of the string or other sound-producing body be *rapid*, the *waves* will be *short* and the pitch of the sound correspondingly *high*. The undulatory theory teaches that these *air-waves* are moulded by the string or tuning-fork into "condensations and rarefactions," and sent off in this form to the ear, however distant so the tone is audible, producing the sensation of sound *by the successive dashing of these air-waves against the tympanic membrane*, thus causing the drum-skin of the ear to oscillate synchronously to such waves. Hence, that these *air-waves*, moulded and sent off by the vibrating string or fork, ***must travel undistorted the entire distance the sound is heard, it matters not what counteracting currents, waves,*** *sounds, or atmospheric disturbances may cross their path!*

Perhaps there is no better place than right here to make a few brief citations from the highest living authorities on this subject, in order that the real position of scientists on the current wave-theory may not be misunderstood. These citations are selected because they concisely embody the popular notions regarding sound-waves, with an authority which is looked up to as standard in all our institutions of learning. I request the reader to carefully read them; and, if not familiar with this branch of scientific investigation, to study them, as a proper comprehension of their teaching will save time in prosecuting the argument, and prevent the necessity for frequently recurring to this list of passages. All my quotations from Professor Tyndall's *Lectures on Sound*, in the course of this argument, will be made from the second edition, except in a few instances from the third edition, which will be indicated. This occurs for the reason that most of the arguments were prepared before the third edition of *Lectures on Souna* was published. Professor Tyndall remarks as follows:—

1.—"With regard to the point now under consideration, you will, I trust, endeavor to form a definite image of a *wave of sound.* You ought to see mentally the *air-particles* when urged outwards by the explosion of our balloon crowding closely together; but immediately behind this *condensation* you ought to see the *particles separated more widely apart.* You ought, in short, to be able to seize the conception that a *sonorous wave* consists of two portions, in the one of which the *air is more dense* and in the other of which *it is less dense than usual.* A *condensation* and a *rarefaction*, then, are the two constituents of a *wave of sound.*"

2.—"Fix your attention upon a *particle of air as the sound-wave passes over it;* it is urged from its *position of rest towards a neighbor particle*, first with an *accelerated motion* and then with a retarded one. The *force* which first urges it is opposed by the elastic force of the air, which finally stops the particle, and causes it to recoil. . . . The *distance* through which the air-particle *moves to and fro, when the sound-wave passes it,* is called the *amplitude of the vibration.* The *intensity* [loudness] of the sound is also proportional to the *square of the amplitude.*"

3.—"The motion of the *sonorous wave* must not be confounded with the motion of the *particles* which at any moment *form the wave.* During the passage of the *wave every particle* concerned in its transmission makes only a *small excursion to and fro.* The *length of this excursion* is called the *amplitude of the vibration.*'

4.—"A sonorous wave consists of two parts, in one of which the *air is condensed*, and in the other of which *rarefied.* . . . In the *condensed* portion of a sonorous wave the *air is above*, in the rarefied portion it is *below the average temperature.* . . . *This change of temperature produced by the passage of the sonorous wave itself virtually augments the elasticity of the air, and makes the velocity of sound about one sixth greater than it would be if there were no change of temperature.* . . . When I speak of a sonorous wave I mean a *condensation* and its associated *rarefaction.* . . . When a body capable of emitting a musical sound—a tuning-fork, for example—vibrates, it *moulds the surrounding air into sonorous waves*, each of which consists of a *condensation* and a *rarefaction.*"

5.—"We have already learned that what is *loudness* in our sensations is *outside of us* nothing more than *width of swing* or *amplitude* of the vibrating *air-particles.*"

6.—"Having determined the rapidity of vibration, the *length* of the corresponding *sonorous wave* is found with the utmost facility. Imagine this tuning-fork vibrating in free air [384 vibrations to the second]. At the end of a second from the time it commenced its vibrations, the *foremost wave would have reached a distance of* 1090 *feet* in air at the freezing temperature. In the air of this room, which has a temperature of about 15° Cen., it would reach a distance of 1120 feet in a second. *In this distance*, therefore, are embraced 384 *sonorous waves.* Dividing, therefore, 1120 by 384, we *find the length of each wave to be nearly* 3 *feet.*"

7.—"How are we to picture to ourselves the *condition of the air* through which this *musical sound* is passing? Imagine one of the prongs of the vibrating fork swiftly advancing; it *compresses the air* immediately in front of it, and when it retreats it leaves a partial vacuum behind, the process being repeated by every subsequent advance and retreat. The whole function of the tuning-fork is to *carve the air into these condensations and rarefactions*, and they, as they are formed, propagate themselves in succession through the air. A *condensation* with its associated *rarefaction* constitutes, as already stated, a *sonorous wave*. In *water* the *length of a wave* is measured from *crest to crest;* while in the case of sound the *wave-length* is given by the *distance between two successive condensations*. In fact, the *condensation* of a sound-wave corresponds to the *crest*, while the *rarefaction* of the sound-wave corresponds to the *sinus of the water-wave*."

8.—"Figure clearly to your minds a harp-string vibrating to and fro; it advances, and causes the *particles of air* in front of it to *crowd together*, thus producing a *condensation of the air*. It retreats, and the *air-particles* behind it separate more widely, thus producing a *rarefaction of the air*. The string again advances, and produces a condensation as before; it again retreats, and produces a rarefaction. In this way the *air* through which the *sound* of the string is propagated is *moulded* into a regular sequence of *condensations* and *rarefactions*, which *travel* with a velocity of about 1100 feet a second. The *length of the wave* is measured from the *centre* of one condensation to the *centre* of the next one."

9.—"We must devote a moment's attention in passing to the word 'amplitude,' here employed. The pitch of a note depends solely on the number of *aerial waves* which *strike the ear* in a second. The *loudness* or intensity of a note depends on the *distance* within which the separate *atoms of the air vibrate*. This *distance* is called the *amplitude* of the vibration."—TYNDALL, *Lectures on Sound*, pp. 5, 11, 44, 46, 48, 62, 69, 83.—*Heat as a Mode of Motion*, pp. 225, 372.

I also quote from Professor Helmholtz:

10.—"Suppose a stone to be thrown into a piece of calm water. Round the spot struck there forms a little ring of wave, which, advancing equally in all directions, expands to a constantly increasing circle. *Corresponding to this ring of waves sound also proceeds in the air* from the excited point, and advances in all directions as far as the limits of the mass of air extend. *The process in the air is essentially identical with that on the surface of the water*."—HELMHOLTZ, *Sensations of Tone*, p. 14.

I have numbered the foregoing citations in view of possible reference to them as the argument advances.

With these passages before the reader there need be no difficulty in grasping the essential features of the wave-theory of sound, which, in fact, up to the present moment, is the only hypothesis ever advanced, so far as I have been able to learn, by which to explain these well-known phenomena. Other passages will be quoted, from time to time, as special questions come up for discussion.

Believing, as I do, that the new hypothesis of sonorous discharges of some sort of attenuated substance will fully and satisfactorily explain all phenomena observed in sound, even better than they can be explained by physical and mechanical air-waves, I will at once make a practical application of the corpuscular theory to a few problems which have been always looked upon as conclusive proof of the air-wave hypothesis.

The first and one of the most prominent examples of this kind is that of *sympathetic vibration*, or the surprising fact that if two strings or forks are tuned to perfect unison or in such a way that they will make exactly the same number of normal oscillations in a second, and if one of them is thrown into vibration, its unison neighbor if placed near enough to it will also start into vibratory motion, and sound audibly without any connection whatever with the actuating string or fork except the intervening air.

The reason assigned for this by the advocates of the current theory, is, that the air-waves moulded and sent off from the excited string or fork, striking against its unison neighbor in synchronism with its own normal tendency to swing, start it gradually into oscillation, very feebly at first, but each succeeding air-wave dashing against it in perfect periodicity to its own vibrations, gives it a new impetus at every blow, till finally this sympathetic motion is brought to its maximum. This phenomenon, first observed by Pythagoras over twenty-five hundred years ago, was, perhaps, the origin of the atmospheric wave-theory, since which time it has reigned supreme, never having been called in question by any succeeding investigator of sound. It is, therefore, a venerable and highly respectable theory with which I have undertaken to deal in this discussion.

Though I shall undertake to show that the above explanation can not be the true solution of this sympathetic problem, and that it must be, therefore, a clear mistake based on superficial observation, yet, before doing so I will gradually prepare the reader for the new solution of this singular physical effect, that the two explanations may be placed in juxtaposition before him.

I assume that there is a veritable sympathetic attraction potentially existing in every sound-producing body for every other sound-producing body which has or may have a unison or synchronous vibration. The unison condition alone develops this sympathetic attraction into practical operation. As the analogue of this there exists potentially in every piece of iron magnetic attraction for every other iron body. When a piece of iron is converted or *tuned* into steel, and assumes the character of a magnet through the influence of electric currents, it may be said to be in unison with the molecular character of other iron bodies, causing an affinity to co-exist between them. Why it attracts another mass of iron, overcoming its inertia and causing it to change positions when made to approach it, science does not tell us, yet it is absolutely certain that some kind of substantial currents pass off from the magnet to seize hold of the iron armature or the corporeal result of lifting it could not occur, according to all known physical laws, since it would be an actual physical result caused by *nothing*. We simply know, also, that these substantial currents sent out from the magnet do not move or lift the iron by means of air-waves or the undulatory motions of any intervening substance whatever, as they will pass through platinum, gold, or sheets of water, without the slightest disturbance of their particles, and still move the iron beyond them by some intangible cords connecting them. We know, further, that this magnetic substance, whatever it is, passing from the poles of the solid steel magnet, will not act in the slightest degree on any other body except iron, which alone responds to it sympathetically, just as a sounding string has no sympathetic attraction for any other body, and will stir no other object, however delicately balanced, unless it be a sound-producing body tuned synchronously to its own vibratory swing. There is nothing more mysterious, therefore, or difficult to accept, in a string sending off sonorous pulses of some kind of substantial atoms (which may sympathetically impinge upon the same potential substance in its unison neighbor, causing it to move by synchronously acting upon it and gradually adding to its momentum, the same as air-waves are supposed to effect it) than there is in believing in the almost analogous attraction of the magnet, with which every scientific student is familiar. Scientists do not pretend to explain why magnetic currents will move a piece of iron and nothing else; neither do I claim to know why the substantial pulses from a string will pass off and sympathetically influence a musical body which is in a certain condition and will move nothing else. We simply know that both phenomena exist in Nature. One of them—the magnet—no physicist pretends to explain; while the other, from the most superficial misconception, as I will now show, we are told is easily explicable by the synchronous dashing of literal air-waves against it, as you might also start it by successive blows from a stick dealt with suitable periodicity.

As a proof that the sympathetic vibration of a unison body is not caused by the periodic impulses imparted to it through air-waves sent off from the actuating string or fork, I refer the reader to the unanswerable fact that a body may vibrate or oscillate ever so nearly to another body tuned in perfect synchronism with its own swing, and ever so rapidly, but so long as no audible tone is produced by these vibrations no motion whatever will be communicated to the unison neighbor, though it necessarily and continuously receives the synchronous air-waves driven against it by the actuating body. I have carefully tested this in the following manner: I arranged two pendulum balls, with very short rods of equal length, to cause rapid swings as closely together as possible without touching, being careful that their supports had no immediate connection (except the air) by which any impulse might be communicated from the moving ball to the one at rest. Though their swings were in perfect synchronism, *moving with twice the aggregate velocity of a tuning-fork's prongs*, and although they were so near together that the air-disturbances caused by the moving pendulum must necessarily strike the other periodically, or as nearly so as it is possible for air-waves to travel, yet no motion whatever was communicated to the one at rest, for the best of all possible reasons—*there was no tone produced.*

This is also illustrated in the case of a sonometer-string, if taken from its sounding-board and stretched over isolated pieces of rigid iron; though it will vibrate when plucked just the same, and "carve" or "mould" the air into waves, as Professor Tyndall expresses it, just to the same extent exactly as when in connection with its sounding-tray, yet its sound can scarcely be heard by a person standing near it, for the want of a resonant body to augment its tone by diffusion, as will be explained after a little. A string in this condition will not start a unison body into sympathetic vibration even if but a few inches distant, and then only in exact proportion to the intensity of its sound, and not at all in proportion to the amplitude of the air-waves "moulded," "carved," and sent off by its oscillations, which are exactly the same whether such string is connected with the sounding-board or not. If the air-waves are really moulded and sent off by the harp-string, with "condensations and rarefactions" traveling 1120 feet a second, as so explicitly taught by Professor Tyndall (see extracts 7 and 8, pp. 78, 79), and if these air-waves are really the cause of sympathetic vibration in a distant unison string or fork, then pray tell us why the sonometer-string can cause no response to its unison neighbor a foot from it, though it "carves," "moulds," and sends off the same air-waves it does when placed on its sounding-board? The air-wave hypothesis must therefore completely break down as the solution of sympathetic vibration.

Professor Robert Spice, of 230 Bridge Street, Brooklyn, N. Y., the foremost acoustician and one of the most careful and painstaking investigators of sound in this country, informs me that he has made tuning-forks which, when mounted on accurate resonant cases, have responded to each other sympathetically at a distance of 180 feet apart. Such forks, disconnected from their resonant cases and consequently deprived almost entirely of sound, would not cause the slightest sympathetic effect upon each other if held but an inch apart, simply for the want of effective tone, notwithstanding the air-waves "carved" and "moulded" by the prongs of the fork are exactly the same in the one case as in the other. Something else, then, evidently, besides air-waves sent off from an oscillating instrument is required to account for sympathetic vibration.

But the advocate of wave-motion is here ready with an objection. He urges that in placing the fork or string into contact with the sounding-board the vibrations of the instrument are vastly multiplied by the greater surface of the board, producing thereby a greater effect upon the air, or, in other words, sending off more powerful air-waves than can be sent by the fork or string alone, and that these supplementary air-waves, caused by the vibratory motion of the sounding-board, are the real cause of the sympathetic response of a unison instrument at such a great distance.

This view of the case at first sight would seem to have some weight; but when carefully looked into it will be found to be based on a misunderstanding of the laws governing resonance. It will therefore be necessary to devote a few pages to this somewhat complex question, and thus try to explain the true function of sounding-boards, resonant cases, &c., in connection with musical instruments, at the same time correcting a number of superficial but palpable errors of physicists.

As an evidence that the advocates of the wave-theory of sound have no clear conception of the phenomenon of resonance,—attributing it, as they do, to a simple increase in atmospheric disturbance, or to an augmentation of air-waves,—we have only to note their flat and unavoidable contradictions when treating on different phases of their theory. The reader will be made fully aware, before this treatise is concluded, that the profoundest and most careful investigators of sound-phenomena are unavoidably forced to contradict themselves and the elementary principles of the wave-theory in numerous ways, simply because the theory itself is intrinsically erroneous, and based on a pure misconception of natural laws; hence, in dealing with different aspects of the subject, its ablest advocates are necessarily and naturally led into the most preposterous absurdities and laughable incongruities.

In explaining "sonorous waves" to his audience, and in what manner they are sent off from a vibrating string through the sounding-board of a sonometer, Professor Tyndall remarks:—

"The *sonorous waves* which at present *strike your ears* do not proceed *immediately from the string*. The *amount of motion* which *so thin a body imparts to the air is too small to be sensible at any distance*. But the string is drawn tightly over the two bridges, and when it vibrates its tremors are communicated through these bridges to the entire mass of the box."—*Lectures on Sound*, p. 87.

He next experiments with a similar string without any kind of a sounding-board, it being merely stretched over rigid pieces of iron, and remarks:—

"I now pluck the string. It *vibrates vigorously, but even those on the nearest benches do not hear any sound. The agitation which it imparts to the air is too inconsiderable to affect the auditory nerve at any distance*. . . . It is not the *chords of a harp*, or a *lute*, or a *piano*, or a *violin*, that *throw the air into sonorous vibrations*. It is the *large surface with which the strings are associated*."—*Lectures on Sound*, p. 88.

Professor Helmholtz, admitted to be the leading physicist on sound in Europe, teaches precisely the same doctrine in regard to the resonance of a sounding-board, and it was no doubt from his work on the *Sensations of Tone* that Professor Tyndall caught the above inspiration. This great German authority says, in speaking of the resonant effects of sounding-boards:—

"As we have had already occasion to remark, *vibrating strings do not directly* communicate any *sensible portion of their motion to the air*."—*Sensations of Tone*, p. 137.

Here, then, while declaring that it is not the air-waves from the string which we hear, since a string is "so *thin* a body" that its waves are not "*sensible* at any distance," Professor Tyndall forgets his explicit argument quoted on page 79, extract No. 8, in which he says:—

"Figure clearly to your minds a *harp-string* [which he says here can not "throw the air into *sonorous vibrations*"!] vigorously vibrating to and fro; it advances, and causes the particles of air in front of it to crowd together, thus producing a *condensation of the air.* It retreats, and the air-particles behind it separate more widely, thus producing a rarefaction of the air. . . . *In this way* [Mark it, "in *this way*," by the simple motion of the harp-string, without a word about its sounding-board advancing and retreating!] *the air through which the sound of the string is propagated is moulded into a regular sequence of condensations and rarefactions* which travel with a velocity of about 1100 feet a second."

Thus, in one breath he teaches that the air-waves are due entirely to the motions of the *string*, which moulds and sends them off at a velocity of 1100 feet a second; then, in the next, it is just as explicitly taught that "so thin a body" as a string can not produce sound-waves which would be "sensible at any distance"; and finally, to make the contradiction as flat as possible, he adds: "*It is not the chords of the harp . . . that throw the air into sonorous vibrations.* It is the large surface with which the strings are associated"!

A theory based on a correct understanding of the physical laws surely would not thus so palpably contradict itself. No better proof need be required by the unscientific reader that a theory is radically defective, if not intrinsically false, than to see such incongruous statements as to its fundamental principles when being presented by its ablest advocates. If its various phases will not hold together and harmonize, the theory must be false.

But is this transferrence of the vibratory motion of the string to the sounding-board, thus causing it to act on the atmosphere and send off augmented air-waves, the true solution of this problem of resonance? By a little reflection it will be seen that the sounding-board can not, by any possibility, aid the string by augmenting its sound, if such augmentation depends on air-waves generated by the motions of the board, and for reasons which I will now try to show are clearly unanswerable.

In the first place, the pitch of a tone, as every one admits, depends on the number of vibrations per second of the sounding body. In the second place, the tone of a string never changes its pitch in being transferred to and augmented by the sounding-board; and though the board necessarily receives a tremor from the vibrating string bearing against it, such tremor can only be regarded as incidental, or as *the effect of the motion which produces the tone*, and not such motion itself. It is this fundamental and manifest error of supposing an incidental or fortuitous effect of sound to be actually the cause of the tone which has done more than anything else to keep the wave-theory so long in existence.

As the sounding-board of an instrument often produces a hundred-fold augmentation of tone compared to that of the naked string, it is perfectly evident that this vast increase of sound can not be the result of corresponding increase of vibratory motion and of air-waves sent off, as the wave-theory unavoidably teaches, since this would necessarily make the sounding-board the controlling mechanism in the production of tone; and consequently, instead of playing a secondary part to the string, which has but a hundredth part the vibratory effect on the air, the board should at once take possession of the sound, and change its pitch to its own vibratory rate!

Is it reasonable to suppose, if resonance, producing a hundred-fold augmentation of tone, is caused by the vibration of the sounding-board and by the air-waves sent off from it, that its normal vibratory oscillation would be under the control of the string's trifling vibration, which, unassisted, can not make a hundredth part of the sound? Is it not clear that the superior mass, surface, and power of the board would assert their right to be heard, and instantly change the pitch of the tone from the string's normal rate to that of the vibrating body whose waves actually produce a hundred-fold more tone? If the wave-theory be correct, that resonance is really caused by the vibratory motion of the board, then evidently each string as soon as sounded should lose its own identity and be forced to conform to the normal pitch of the sounding-board. This wave-theory of resonance involves the startling inconsistency of a vibrating body, having a hundred-fold more power over the air, being coerced out of its own normal oscillation into an abnormal and obnoxious swing which causes a hundred-fold the amount of tone, while the string itself, not a thousandth part as large in area, retains its perfect pitch, mastering and annihilating that of its powerful coadjutor! As an effect so vast could not, by any possibility, be produced by such an inadequate cause, it follows that the resonance produced by a sounding-board must receive some other explanation than that given by the wave-theory.

The well-known comical illustration of the wagging of a dog's tail, though somewhat ludicrous, is so completely applicable to this case, and every way so mechanical and appropriate, that I am obliged to refer to it. The inquiry why a dog wags his tail was philosophically answered, because the tail was the *smallest*, or otherwise the tail would wag the dog! The theory of resonance, as taught by Professor Tyndall, inverts this sensible answer, and makes the diminutive "tail" of a string wag the enormous "dog" of a sounding-board, at the same time giving it a hundred-fold more wagging motion than it has to communicate! Surely an explanation so palpably absurd can not be the correct one.

That the tremor of the sounding-board, or the movement it may impart to the air, is only incidental, or a fortuitous effect of the actual cause of the sound itself in the motion of the string, just as the recoil of a cannon or the disturbance of the surrounding atmosphere thus produced at its discharge, is but incidental to the projectile's movement, and no part, necessarily, of such propulsion, will be made clear in a moment to the most unscientific reader.

The sounding-board of the piano, for example, has *eighty-five* separate sets of strings bearing against its surface, each of which has a different rate of vibration of its own, and consequently a separate pitch of tone. Now, while the sounding-board does really augment by resonance the sound of each of these eighty-five sets of strings, it has, as just intimated, but one normal rate of vibration of its own, and if bowed across its edge will produce but one pitch of tone—a heavy, low, and dull sound. Yet, if the eighty-five sets of strings, with eighty-five distinct rates of vibration and pitches of tone, were all to be sounded at one time, the board would nevertheless resound to every string at the same instant, while not the slightest change would occur in the pitch of tone or rate of vibration in either of the sets of strings! The wave-theory, in attempting a solution of resonance, in the case of a pianoforte, is thus forced to assume that a single board, with but one normal rate of vibration, is capable of sending off from its surface no less than eighty-five separate systems of air-waves (as the real and only cause of the tones we hear, according to Professor Tyndall), each system having a different rate of vibratory motion, and oscillating with a different amplitude of swing at the same instant of time, and all save one forced or coerced away from the normal oscillation of the board, since the distinct note of any one set of strings can be sorted out from the entire mass of tone, even when all the strings are sounded together, if the ear is aided by a suitable resonator tuned accurately to that particular note!

The mere presentation of such a physical and mechanical impossibility (since aerial waves are nothing but the result of physical and mechanical forces and operations) ought to be sufficient to cause any properly trained, analytical mind, to at once reject a theory the truth of which has to depend on such a result.

No well-informed advocate of the current hypothesis of sound will pretend to call in question the truth of the position here stated, namely, that if the wave-theory be true, it must be possible for the surface of a single sounding-board to be thrown at one time into eighty-five distinct systems of undulations, all different in amplitude and rates of oscillatory motion, each rate of vibration sending off a system of air-waves corresponding in width of swing and periodic time to that particular undulation of the board, each causing a counter condensation and conflicting direction to the same air-particles, the whole eighty-five systems of waves occupying the same air of the same room at the same time, and each wave passing through it undistorted and independently of the other eighty-four systems, the same as if they were not at that very instant permeating the atmosphere!

Now, if I am able to show from the highest living authority on sound, as well as on all questions involving the operations of the physical laws, that these eighty-five different systems of vibratory motions and resultant air-waves, with their conflicting amplitudes, periodic rates, condensations and rarefactions of the air, or even two such systems, are wholly impossible and out of the question in the same atmosphere at the same time, must not the theory based on such a mechanical result be utterly shattered? I have at hand, fortunately, just such a conclusive and sweeping overthrow of the very foundation of the wave-theory from the pen of no less an authority than Professor Helmholtz himself, which the reader, if he be a believer in the wave-theory of sound, is requested particularly to note:—

"It is evident that at *each point* in the mass of air, at *each instant of time, there can be only one single degree of condensation*, and that the *particles of air can be moving with only one single determinate kind of motion, having only one single determinate amount of velocity*, and passing in only *one single determinate direction*."—*Sensations of Tone*, p. 40.

And immediately after this, as if the foregoing language was not sufficiently strong to annihilate the wave-theory, the Professor adds:—

"It is *true* that *two different degrees of density* produced by *two different systems of waves can not co-exist in the same place at the same time*."—*Sensations of Tone*, p. 42.

How, then, could eighty-five distinct and separate systems of undulations coexist in the same air and pass off from the same surface of the sounding-board at the same instant of time, each system of waves of a different "condensation" or "density," as would be the case if there was the slightest difference in the intensity of the tones, since each wave produces a condensation of the air exactly in proportion to its loudness or the "width of swing" of its air-particles?

If there is any meaning in words, my position is fully sustained; for, if Professor Helmholtz had aimed to crush out the wave-theory of sound at a single blow and show its utter untenability, and particularly the idea of resonance consisting in augmented air-waves, he could not more effectually have accomplished his work than he has done in the above unnecessarily emphatic negation of the entire hypothesis.

To strengthen this view, that the tremor of the sounding-board and its resultant air-waves are but incidental, and not the cause of the great augmentation of the tone heard, it is a fact, proved by the beautiful experiment of Professor Wheatstone, that all the tones of the piano can be condensed and conducted longitudinally through a long slender rod, by letting one end of it rest on the sounding-board and placing a violin against the other; and I can not resist the temptation of here quoting bodily the beautiful description of this experiment given by Professor Tyndall in one of his lectures:—

"We are now prepared to appreciate an extremely beautiful experiment, for which we are indebted to Professor Wheatstone, and which I am now able to make before you. In a room underneath this, and separated from it by two floors, is a piano. Through the two floors passes a tin tube 2½ inches in diameter, and along the axis of this tube passes a rod of deal, the end of which emerges from the floor in front of the lecture-table. The rod is clasped by india-rubber bands, which entirely close the tin tube. The lower end of the rod rests upon the sound-board of the piano, its upper end being exposed before you. An artist is at this moment engaged at the instrument, but you hear no sound. I place this violin upon the end of the rod; the violin becomes instantly musical,—not, however, with the vibrations of its own strings, but with those of the piano. I remove the violin, the sound ceases; I put in its place a guitar, and the sound revives. For the violin and guitar I substitute this plain wooden tray; it is also rendered musical. Here, finally, is a harp, against the sound-board of which I cause the end of the deal rod to press; *every note of the piano is reproduced before you.* I lift the harp so as to break its connection with the piano, the sound vanishes; but the moment I cause the sound-board to press upon the rod, the music is restored. The sound of the piano so far resembles that of the harp that it is hard to resist the impression that the music you hear is that of the latter instrument. An uneducated person might well believe that witchcraft is concerned in the production of this music.

"What a curious transferrence of action is here presented to the mind! At the command of the musician's will his fingers strike the keys; the hammers strike the strings, by which the rude mechanical shock is shivered into tremors. The vibrations are communicated to the sound-board of the piano. Upon that board rests the end of the deal rod, thinned off to a sharp edge to make it fit more easily between the wires. *Through this edge*, and afterwards *along the rod*, are poured with unfailing precision the entangled pulsations produced by the shocks of those ten agile fingers. To the sound-board of the harp before you the rod faithfully delivers up the vibrations *of which it is the vehicle.* This second sound-board *transfers the motion to the air, carving and chasing it into forms so transcendently complicated that confusion alone could be anticipated from the shock and jostle of the sonorous waves.* But the marvellous human ear accepts every feature of the motion; and all the strife and struggle and confusion melt finally into music upon the brain."—*Lectures on Sound*, p. 80.

Had the wave-theory of sound not been assailed as utterly inadequate to account for this wonderful transferrence of the complicated sounds of the piano through the length of this rod by means of corresponding wave-motions, having each a separate rate of vibration and width of swing, we might still go on believing in such "witchcraft"; but the evidence a moment since quoted from Professor Helmholtz, proving that no two systems of waves—of different densities, of different rates of motion, and of different amplitudes,—can co-exist in the same place at the same time, is a sufficient proof that the incidental up and down tremor of this deal rod resting against the sounding-board is not and can not be the true cause of communicating so many complex musical tones to the violin at the same instant. Besides, the explanation of Professor Tyndall is completely overthrown by substituting an *iron rod* for the one of *deal*. Such a rod receives the same tremor precisely from the sounding-board of the piano, and communicates it just as effectively to the violin,—as it surely ought to do, being a fourfold swifter conductor of sound than wood. But no music whatever is heard by the audience. If the *vibratory motion* of the sounding-board, thus transferred longitudinally through a rod to the violin, is the true cause of this resonance, then manifestly the music should be the same through the iron rod as through the deal, since the vibratory motion is essentially the same in both cases.

But in dealing with this question of resonance, which really lies at the foundation of the wave-theory, and which, if it can be satisfactorily explained without air-waves, overthrows the entire hypothesis, I am not left to simple argumentation based upon facts, however strongly they may bear against the current explanation. I am not even obliged to rest on the explicit admission of Professor Helmholtz just quoted, or the self-contradictory statements of Professor Tyndall, as shown at the commencement of this argument on resonance, in which he assures us that the harp-string both makes the tone and don't make it! I have at hand a simple and unquestionable demonstration, in the form of a single experiment within the reach of any one desiring to test it, which shows beyond the shadow of a doubt that the resonance of a sounding-board has nothing whatever to do with its incidental tremor or the air-waves thus produced, which, if it turns out as I now state it, alone breaks down the wave-theory.

This experiment consists in holding the stem of a large tuning-fork in contact with a dry pine chip of about the same bulk, which will cause a resonant augmentation of the tone of the fork at least twofold. Now, while the prongs of the fork can be plainly seen to oscillate a sixteenth of an inch, sending off corresponding air-waves, the chip is destitute of all visible vibration, and consequently can send off no appreciable air-waves as compared to those generated by the fork, *notwithstanding it doubles the volume of sound by resonance!* Professor Tyndall says the air-waves moulded and sent off from the fork do not cause the sound we hear, but it is caused by the waves generated by the *large surface* of the sounding-board against which the fork is held! Will the Professor tell us how it is when the surface of the board is no larger than that of the fork, while the sound is doubled, with not over one-fifth the vibratory motion? For it is perfectly manifest that the chip against which the stem of the fork is held can only receive a vibratory motion equal to the up and down motion of the stem, which can be but a very small fraction of that of the prongs laterally; and consequently, if air-waves be the secret of sound-production, the augmentation by the motion of the pine chip should not be appreciable.

Can these advocates of the wave-theory, who draw sage conclusions on profound scientific questions from a few superficial observations, tell us how this pine chip, with not over one fifth the oscillatory motion of the fork's prongs, can produce a twofold augmentation of the sound by the generation and propagation of air-waves, while the fork's five or ten fold oscillation, with a five or ten fold aerial disturbance, can not be heard "at any distance," as Professor Tyndall himself assures us?

As in the case of the sounding-board of the piano, there is unquestionably an incidental tremor communicated to the chip by the movement of the fork, which can be felt by the hand, though too infinitesimal to be seen. I stated on page 83 that this tremor of the sounding-board was only *incidental, as the result of the motion which produced the tone, and not its cause.* I will now prove it so clearly that a child can see it. If the *tremor* of the chip really is the cause which produces the augmentation of tone by moulding and carving the air into sonorous waves, then any other body of the same size, substituted for the chip, which necessarily must receive exactly the same tremor when in contact with the stem of the tuning-fork, would necessarily produce the same augmentation of tone, as just shown by substituting an *iron* for a *deal* rod in the Wheatstone experiment, because it would necessarily generate and send off the same amplitude and number of air-waves. So far from this being the fact, if we hold a piece of iron of the size of the chip against the stem of the fork, not the slightest increase of tone occurs, though the iron is felt to tremble exactly the same as the chip, even more so, being more firm and elastic. Here, then, we have all the vibration in the piece of iron that we had in the chip, and consequently all the additional air-waves sent off without a particle of augmented sound! To say that this utterly shatters the wave-hypothesis and Professor Tyndall's explanation of a sounding-board's resonance, is to say what the common sense of every reader has already admitted.

We can go even further in regard to the tremulous motion of the chip, or its iron substitute, caused by the up and down motion of the stem of the fork while the prongs are vibrating laterally. By means of a very delicate calculation and experiment made by Professor Robert Spice, as explained in a paper published in the *American Journal of Science* for December, 1876, the vibration of the stem of the fork vertically in proportion to that of its prongs laterally is clearly stated. The Professor found, by careful examination and measurement, to which he has called my attention, that a fork whose prongs oscillate a *sixteenth* of an inch communicates an up and down synchronous movement to its stem of *one eightieth* of an inch, or exactly one fifth of its lateral oscillation. Thus, in another and unexpected way, and by impartial scientific testimony, we demonstrate the fallacy of the air-wave explanation of resonance; for, while the fork's prongs oscillating a sixteenth of an inch can not be heard "at any distance," as Professor Tyndall says, though they necessarily produce considerable atmospheric disturbance in their immediate vicinity, yet the stem moving up and down but *one eightieth* of an inch, doubles the sound acting on a chip no larger than the fork, while the iron substitute having the same motion precisely and generating the same air-waves at the same rate per second and of the same amplitude, does not add an iota to the normal sonorous effect of the naked fork!

Is it not, then, overwhelmingly established, from these several considerations, that the advocates of the wave-theory are entirely mistaken as to the cause of resonance in a sounding-board? If they are thus mistaken, then, evidently, the wave-theory itself is left without a foundation on which to rest; for, if resonance can occur without the generation of corresponding air-waves, as we here see it can, so can any other tone ever produced!

But now we come to the important question, if the resonance of a sounding-board by which the tone of a string is augmented ten, twenty, or an hundred-fold, be not caused by its incidental tremor or by air-waves sent off, as we see it is not and can not be, then is there any probable or reasonable solution of this phenomenon? I answer, there is; and I will now try to make the reader understand it.

Resonance is of two kinds. One kind consists in the radiation or diffusion of tone from a body such as a piano sounding-board, where effectiveness depends on two principal conditions, namely, the molecular structure of the body itself and the extent of its surface, including also its form, partly, and its manner of support; while the other kind of resonance consists in the sympathetic vibration of a column of air tuned to perfect synchronism with the sounding body which excites it into action.

In the first-named variety of resonance are included all sounding-boards, such as those of pianos, harps, violins, sonometers, guitars, &c. In the second belong wind-instruments of all kinds, organ-pipes, flutes, horns, &c.; for the agitation of the air at the mouths or debouchures of these instruments, even when caused by the lips or by reed-motion, becomes the sound-generator, while the air in the horn or resonant pipe-chamber is made to express and augment the tone by its own resonant or sympathetic vibration.

To this class, also, belong resonant cases used for mounting tuning-forks, whose air-chambers, to be effective, should be of such a depth and capacity as to give forth its loudest resonance when the tuning-fork intended for it is sounded over its open mouth.

Advocates of the wave-theory, including Professors Tyndall and Helmholtz, assume and teach that the loudest resonant depth of such a case, in feet and inches, is exactly and invariably *one quarter of a wave-length* of the sound thus most loudly augmented. If this were so, it would be a remarkable coincidence, and go strongly to confirm the truth of the wave-theory; and it is a real pity to take from the hypothesis what seems to be absolutely its only collateral support, which will be done most effectually in the following chapter, when we come to the review of Professor Tyndall's famed lectures on sound.

Professor Spice, as before intimated, has constructed two unison forks, and mounted them on accurate resonant cases 180 feet apart, and caused one of them to speak sensibly by exciting the other with a violin-bow. How is this result effected?—and by what philosophical or physical law is corporeal motion generated in the distant fork by sounding its unison so far from it? The wave-theory has no practical solution to offer (being a purely physical and mechanical hypothesis, depending on the momentum of corporeal air-waves, with all their inertia and friction to be overcome), and can suggest nothing except that these air-waves are actually driven off the entire distance by the motions of the actuating fork and its resonant case; and that such aerial undulations, after traveling this distance, are successively dashed against the fork and its case till oscillation is gradually brought about, as recently explained.

This solution is manifestly absurd and impossible; and any scientific student would instantly see it should he reason on air-waves as he would reason on water-waves or any mechanical result requiring physical force and the overcoming of friction and inertia by momentum to effect it. Simple air-waves, or any other forms of aerial disturbance, can not move through the surrounding atmosphere, in its quiescent condition, except at a very slow speed and to a very limited distance, however they may be put into motion, or whatever force may be exerted in starting them. It is astonishing that such a radical error should be universally taught and believed as that an air-wave started or sent off by a tuning-fork or string should travel on any other principle than if sent off from a fan or the motion of the hand. The prong of a tuning-fork in passing through the air at full amplitude, moves only at a very low velocity, *not one tenth as fast as we can move an ordinary fan*,—a fact perhaps never thought of by a writer on sound; for, if it had been, he surely would have abandoned the wave-theory. This fact will be fully illustrated at the close of the next chapter. But here permit me only to remark that it is mechanically impossible for a vibrating fork to send off air-waves at furthest over a foot or thereabouts from the oscillating prongs, while the velocity of such waves *can not, by any possibility, exceed the velocity of the moving prongs which impell them!*

Professor Tyndall, in the very commencement of his lectures on sound, indulges in such superficial and sophistical reasoning on this question that I can not refrain from pointing it out here. He compares, for example, the action of an air-wave sent off from a vibrating body to that of a *spring*, which, when shoved longitudinally, moves throughout its whole length, though recoiling somewhat under the impelling force according to its elasticity, and leaves the impression on his audience and on the readers of his book that air-particles act precisely in the same way when moved by a vibrating body like a fork or string. A weaker fallacy was never recorded; yet it is just that very logic on which his whole theory depends. Suppose the substance of a spring to be as mobile as air and as easily displaced laterally, what becomes of it when one end is shoved in the direction of its length? If the shoving motion is as slow as that of the prong of a tuning-fork (about seven or eight inches a second), the portion of the spring in front of the impelling body would quietly move around behind as fast as it advanced, thus forming an equilibrium of the spring's substance without stirring it a foot in front! If you move even the broad surface of a fan through the air at a velocity of only eight inches a second, *what becomes of the air in front of it, which is all the spring we have to take into consideration in this discussion?* It simply moves around the fan, quietly and silently taking its place behind it, without causing the slightest disturbance or displacement of these *spring-particles*, so talked of by these learned writers, a dozen inches in front of it!

I have thoroughly and carefully tested this velocity of air-waves and this spring-power of the atmosphere in transmitting condensed pulses, so essential to the wave-theory, by moving the broad side of a stiff fan through it in rapid oscillations, driving it at a velocity exactly ten times greater, by measurement, than that of the vibrating prong of a tuning-fork, and have thus determined the actual distance such air-waves can be made to travel by one-man power in a closed room, as well as their maximum velocity. To the utter discomfiture of the wave-theory, the experiment showed that a delicate and sensitive gas-jet could not be stirred at a distance of more than twenty to twenty-five feet, while it took the most powerful waves I could produce, using all the strength of my arm, *five seconds to travel that distance!* How fast, then, I ask these sagacious scientists and profound thinkers, would the same kind of an air-wave, manufactured on exactly the same principle, travel, driven off from the prong of a tuning-fork, which has but a hundredth part of the surface, and moves with only one tenth the velocity?

If the atmosphere really possesses spring-power at all (which I do not doubt, under proper conditions), and which adds to the velocity of such manufactured air-waves, I surely ought to get *one thousand* times the advantage of it over the tuning-fork, having one hundred times the surface with which to take hold of the air, and ten times the velocity by which to impell the waves; for while the fork, with 128 vibrations a second, moves less than the sixteenth of an inch at a swing, making an entire aggregate of less than eight inches and return in a second, I moved the fan a distance of almost seven feet and back each second, with the result just given.

The truth is, this talk about the spring-power of the atmosphere in front of a fork's prong when slowly shoved, or when the air is not confined and acted on within an inclosed space, and about forcing it into "condensations and rarefactions" by this slow movement, thereby generating sufficient "heat" and "elasticity" to add "one sixth" to the velocity of sound, as claimed by the wave-theory, and as is really essential to its existence, while the air at the same time is perfectly free to move out of the way and not be "condensed," is the silliest nonsense ever indulged in by a scientific or unscientific mind; and conclusively shows either a profound ignorance or an utter disregard of the principles of pneumatics and ordinary mechanics. A man who can and really does believe that by moving the prongs of a tuning-fork through the free air at a speed of only *eight inches a second*, they will so compress or squeeze its particles together as to generate sufficient "heat" and "elasticity" *to add one sixth to the velocity of sound*, as does Professor Tyndall, ought to be excused should he believe in the most miraculous witchcraft as well as in all the gods of heathen mythology at once, which he surely ought to be able to do without dangerously over-taxing his credulity.

The only way to appreciably condense the free air by moving a body through it, is either to employ a very large displacing surface, at considerable velocity, or one, if small, at a very high velocity, as when a bullet is fired from a gun. But it is weaker than folly to talk of producing "condensations and rarefactions," and of generating sufficient additional heat thereby to add one sixth to the normal velocity of sound, all by the movement of a harp-string seven or eight inches a second through atmosphere perfectly free to get out of the way and not be "compressed"! The true solution of this problem of atmospheric spring-power will be given in a short time, when we come to look into the nature and effects of magazine explosions, which I hope will cast some light on this long-obscured question of sound-propagation in connection with the transmission of condensed air-waves.

The superficiality of writers on sound is really immense! They actually suppose, as is evident from their writings, that because a vibrating fork makes a *humming tone*, its prongs must therefore necessarily travel at an enormous velocity, so as to *condense* the free air in front sufficiently to generate additional heat and elasticity, and then retreat so rapidly as to create a rarefaction by causing a partial vacuum! This is no exaggerated statement, as will be abundantly proved in what is soon to follow. Professor Tyndall, in his *Lectures on Sound*, page 62, speaks of the motion of the fork in this way:—

"Imagine one of the prongs of the vibrating fork *swiftly* advancing; it *compresses* the air immediately in front of it, and when it retreats it leaves a *partial vacuum behind*, the process being repeated at every subsequent advance and retreat. The whole function of the tuning-fork is to *carve the air* into these *condensations* and *rarefactions*."

Yet Professor Tyndall never thinks to tell his audience of scientific students that while this prong of the tuning-fork is thus "*swiftly* advancing," *cutting* and *carving* the air, retreating with such rapidity as to leave a "*partial vacuum*," thus generating "condensations and rarefactions" in the open atmosphere, it is absolutely only moving at the snail-like speed of *seven inches a second* in one direction, or fourteen counting both! It is but fair and charitable to say he did not know it, but rather that he really supposed the prong of the fork to be moving at a velocity about equal to that of a rifle-ball, or he never would have indulged in such a ridiculous travesty on science and fact.

But he was probably not so much to blame for this superficial misapprehension, since his great mentor, from whom he takes most of his inspirations on sound, Professor Helmholtz, had repeatedly fallen into the same error. Take, for example, his erroneous contrast of the velocity of a pendulum with that of a tuning-fork's prongs, as follows:—

"The pendulum swings from right to left with a uniform motion. . . . Near to either end of its path it moves slowly, and in the middle *fast*. Among sonorous bodies which move in the same way, only *very much faster*, we may mention *tuning-forks*."—*Sensations of Tone*, p. 28.

Whereas it is a fact, which a smart schoolboy should have been well aware of, that a pendulum which beats seconds when thrown into full oscillation, travels more than 64 inches in one direction, or with *more than four times the velocity of a tuning-fork's prongs*, counting their vibrations in both directions!

Professor Tyndall, again following the lead of Professor Helmholtz, as usual, falls into the same mistake in regard to the velocity of a pendulum's movements. He says:—

"The motion of a common pendulum, for example, is periodic; and, as it swings through the air it produces waves or pulses which follow each other with perfect regularity. *Such waves*, however, are *far too sluggish to excite the auditory nerve*. To produce a musical tone we must have a body which vibrates with the unerring regularity of the pendulum, but which can impart *much sharper and quicker shocks to the air*."—*Lectures on Sound*, p. 49.

How can the prong of a tuning-fork, with *only one quarter the velocity of a pendulum*, "impart *much sharper and quicker shocks to the air*" by dividing up this slower movement into sixteenths of an inch instead of continuing its accumulated motion sixty-four inches at a sweep? And how can this motion of the pendulum be called "sluggish," while the motion of the prong, having but one fourth the velocity, is called "much quicker"?

It seems strange, to say the least, that such careful and profound thinkers should be so easily misled by appearances, though it affords a satisfactory answer to the query why it is that the wave-theory of sound, so clearly a scientific fallacy, should be at the present moment believed in by the ablest minds of the world. It can only be because the theory was originally based on a few such superficialities as I am now pointing out, and which no modern physicist has had the originality or mental independence to see through and expose.

In order to get a clear insight into this actual but deceptive velocity of a tuning-fork's prongs, and thus wipe out this surface idea of their "*swiftly* advancing" oscillations, I have only to take the fork in my hand and swing it bodily through the air back and forth a distance of eight inches, making one complete oscillation each second, in which case I move it just as rapidly as its prongs move when sounding, as a moment's calculation will show, while I produce vastly more mechanical and undulatory effect upon the surrounding atmosphere by the longer oscillations; for, while the sounding prong moves but a very short distance in one direction and then retreats, losing the effect of its forward motion in driving the air into any kind of waves or pulses, I swing it bodily at the same velocity exactly, but by continuing and thus accumulating the motion to a greater distance in one direction without interrupting its action, I evidently must produce a greater mechanical effect on the air in front of it than if the long swing were subdivided up into 128 short motions, with not a particle more distance traveled in the aggregate. One would think that a man with the least mechanical intuition could see this, and, in seeing it, would instantly abandon the wave-theory of sound as a most transparent scientific fallacy.

The law governing the generation of tone by a vibrating fork or string may now be concisely stated as follows:—

It is not the mechanical effect of the numerous short motions back and forth on the surrounding air which generates the tone of a fork or string, but it is the molecular effect of the sudden stops and starts on the atomic structure of the instrument itself, causing thereby the emission of the substantial pulses we call Sound, while the atmosphere, wood, water, or iron, through which they pass is but their conducting medium,—any motion of such medium, caused at the time by the vibration of the sound-producing body, being but incidental.

I call the attention of physicists to this important law, embodying, as I conceive, the true philosophy of the generation of tone, here for the first time announced; and I earnestly solicit their impartial judgment on the subject, in view of what has been and what is yet to be offered against the theory of wave-motion,—which, up to the present time, is the only hypothesis ever framed to solve this difficult problem of sonorous propagation.

Upon these sudden stops and starts of a sounding string or tuning-fork, occurring at the rate of a certain definite number per second, depends the pitch of its tone. As these vibratory swings necessarily but incidentally produce air-waves or atmospheric disturbances in the immediate vicinity of the instrument, it was an easy matter for Pythagoras, 2,500 years ago, to make the superficial observation and draw the weak inference, that, since the wider oscillations of the chord make the louder sounds, hence that the loudness of a tone must also depend on the amplitude of these incidental air-waves, or mechanically on the distance the air-particles swing "to and fro" as the sound is propagated to a distant ear. And marvelous as it may seem, this superficial but erroneous view has continued to prevail to the present time, philosophers still continuing to echo the observation and inference of Pythagoras, that as the string swings greatest when the tone is loudest, hence the loudness of a tone at a distance from the sounding body must necessarily depend on the amplitude of the oscillating air-waves, which, instead of traveling as supposed 1120 feet a second, absolutely do not and can not move away from the string a total distance of more than a dozen inches!

Even as great a philosopher as Professor Helmholtz, observing that the loudest sound occurs when the string has the greatest amplitude, jumps to the same superficial conclusion that this proportional width of swing is transferred to the atmosphere, and continued on through it to a distance, the air-particles oscillating at a less and less width as the sound grows weaker and weaker. He says:—

"We easily recognize [just as Pythagoras did] that the force or loudness of a musical tone *increases or diminishes* with the extent or so-called *amplitude of the oscillations of the particles of the sounding body.* When we strike a string its vibrations are at first sufficiently large for us to see them, and its corresponding tone is loudest. The visible vibrations become smaller and smaller, and at the same time the loudness diminishes. . . . The same conclusion results from the diminution of the loudness of a tone when we increase our distance from the sounding body in the open air, although the *pitch* and *quality* remain unaltered; for *it is only the amplitude of the oscillations of the particles of air which diminishes as their distance from the sounding body increases. Hence, loudness must depend on this amplitude.*"—*Sensations of Tone*, p. 17.

Thus, the greatest physical philosopher of the present time can see no deeper into these beautiful effects than to follow Pythagoras, and suppose that the inertia of four square miles of air can be overcome, and all its particles made to oscillate back and forth a definite distance more than 4,000 times a second by the note of a piccolo flute, thus creating *condensations* and *rarefactions* and generating "heat" sufficient to add "*one sixth*" to the velocity of this sound, *requiring hundreds of millions of tons pressure*, as I will clearly demonstrate before this chapter is ended! This observation of these renowned scientists is just as devoid of foundation in fact or philosophy as that of the little child, which, seeing the *trees swing back and forth farthest as the wind blows strongest*, supposes that this *swinging* of the trees is the cause of the wind rather than its effect! I remember distinctly that this was my earliest scientific impression as to the true cause of the wind, when I was about four years old (I should now be ashamed to have been any older), and so explained it to my sister, who still recollects the same highly philosophical observation, which was at least equal in scientific profundity to these sonorous observations of Pythagoras and Helmholtz.

It really seems that no physicist has been able to look below this surface idea and grasp the thought that the reason why the greater periodic swing of a vibrating chord produces the louder tone is because it generates and radiates a greater quantity of sonorous substance, just as the longer sweep or deeper cut of the harvester's cradle brings down the greater quantity of grain; and that the reason why the sound becomes weaker and weaker as the distance from its source becomes greater, is simply because the sonorous particles, radiating in all directions, naturally and necessarily become sparcer and sparcer the more space they are distributed over, which accordingly involves the fact that a less and less number of these sound-atoms strike the tympanic membrane the farther the ear is from the sound-producing body, just as a less and less number of substantial odorous particles enters the nose the farther it is from the source of the fragrance.

Instead of a conclusion so rational, logical, and every way scientific, though lying beneath the surface, Pythagoras observed the merely accidental air-waves generated by the string, and took all the rest for granted; and although the slightest mechanical intuition should have convinced him that such waves were but incidental, as the effect of the motion which produced the tone and *not its cause*, these palpable and self-evident facts and data were ignored, and the childish hypothesis maintained that these same incidental and meaningless disturbances of the air were absolutely the cause of the tone, and continued on through the dense atmosphere at a velocity of 1120 feet a second, or *nineteen hundred times greater than the motions of the string which gave them their impetus!* But the strangest thing of all is that every writer on sound from that time to the present has continued to hold on to the same preposterous idea.

Physicists, however, who take their inspirations from Pythagoras, or even from the great German investigator, Helmholtz, as does Professor Tyndall, will be certain to fall into the gravest of errors, as just seen in regard to the velocity of a tuning-fork's prongs as compared to that of a swinging pendulum.

For example, take the explanation given by Professor Helmholtz of the manner in which a violin-string oscillates under the action of the bow. A more superficial and inexcusable misapprehension does not occur in any work on physics making the least pretensions to scientific accuracy, though his explanation is a vital one, as will be seen, to the wave-theory in some of its essential features. I will now show this so clearly that the unscientific reader will have no difficulty in comprehending the unenviable plight of this learned authority.

He illustrates the action of a bowed string by the motion of a trip-hammer, which is slowly raised by the mill-work and then let drop suddenly, with vastly greater velocity than it ascended, the mill-work representing the bow, while the falling hammer represents the string. But I will give his own words in full, that the reader may the better see the force of my comments:—

"Among motions which produce musical sounds, that of a violin-string, excited by a bow, would most nearly correspond with this [trip-hammer], as will be seen from the detailed description in Chap. V. The *string clings for a time to the bow, and is carried along by it*, then suddenly releases itself, like the hammer in the mill, and like the latter retreats somewhat, *with much greater velocity than it advanced*, and is *again caught by the bow and carried forward*"!—*Sensations of Tone*, p. 29.

The above remarkable scientific statement is the more astounding when we reflect that Professor Helmholtz is a practical violinist of considerable attainment in the art, as well as one of the greatest acousticians of the present time. Yet he does not seem to know the important fact that if a bow should travel slower than the string's normal oscillation at the place where the hair touches it, as he tells us it always does, *there would be no sound produced, since even an attempted vibration of the string would be instantly checked and interrupted, and its tone destroyed by the slower movement of the hair!* If a string can fly back when released from the rosined hair "with *much greater velocity* than it advanced" or than the bow was traveling, as he distinctly teaches, then it will of course rebound forward again *faster than the bow is moving*, since its motion must necessarily be nearly the same one way as the other, when free to move. How, then, in the name of acoustics and mechanics, is it to be "again caught by the bow and carried forward," since it is already moving "forward" with "*much greater velocity*" than the bow? If Professor Helmholtz is right, the "much greater velocity" of the rebounding string would catch the bow and carry it "forward"! And since the string could not be expected to carry forward the slowly moving bow held in the player's hand, the string itself would of course have to stop. The reader must see that it is an unavoidable necessity for the bow to be always moving with as great velocity at least as the normal oscillation of the string when swinging in the same direction or when flying back after being released from the bow, or otherwise the hair would not carry the string with it, but the string would have to carry the hair; and, as before observed, would stop. Yet this highest living authority on acoustics tells us, as above quoted, that the string of the violin, when momentarily released from the hair, will swing back "with much greater velocity than it advanced," or than the bow was moving, which would necessarily cause it to outstrip the bow at its next swing forward, or else to stop at each backward vibration (which, of course, it could not and would not do), and wait for the slowly moving bow to again pick it up and carry it along!

Now, to enlighten this physicist, for he certainly needs it, let us look at the actual movement of a bowed string mechanically for a moment. The open G-string of a violin makes 198 complete oscillations in a second. By the most accurate observation and measurement it is ascertained that this string does not vibrate in ordinary playing over one *sixty fourth* of an inch at the nodal point, or where the hair rubs it, which is about one tenth of its length, measuring from the bridge, thus making the aggregate velocity of the string at this point, or the whole distance it travels in one direction, but *three inches in a second.* To produce an ordinarily loud tone, therefore, the violinist is compelled to draw his bow at a velocity of at least three inches in a second, or otherwise his lagging bow must of necessity interfere with the string's normal oscillation and tend to check it, thus preventing its tone.

It may be observed, however, that in producing a very soft tone, as in piano passages, the string necessarily oscillates considerably less than when yielding a full sound, possibly not the one half of a sixty-fourth, making an aggregate distance traveled in one direction of *not over an inch and a half* in a second, in which case the bow, pressed very lightly, would only need to travel at a corresponding velocity, and still make a pure tone. Less velocity than this would again destroy it.

It is also true that in producing a very heavy note on the violin (in which case the bow has to be pressed down with considerable force), this G-string will be often observed to oscillate at its center nearly or quite a quarter of an inch, which would make its swing at the nodal point about the twenty-fifth of an inch, or eight inches a second in one direction; but in such a case as this, the violinist is absolutely compelled to move the bow at a velocity of at least *eight inches in a second,* or he will not produce the slightest semblance of a musical tone, though he may, as will be soon explained, move it as much faster as he pleases. If he should drop below this velocity while pressing down the bow sufficiently to cause this large oscillation of the string, the musical tone instantly ceases and degenerates into a horrid scratch which no sensitive ear can endure but for a moment. This scratch occurs for the reason I have already given, by the oscillations of the string being started and prematurely checked before reaching their normal limit by the too sluggish movement of the bow. Any violinist can easily demonstrate the truth of what I am now saying (which equally demonstrates the enormity of the error into which Professor Helmholtz has fallen), that the bow never does and never can travel slower than the string normally oscillates when producing a musical tone. He has only to remember, as the basis of his calculation, that the G-string has just 198 complete vibrations in a second, and then calculate the distance it oscillates.

Now, while the minimum velocity of the bow, to produce pure tone, must of ne sity be equal at least to the velocity of string's normal oscillation (never les Professor Helmholtz says it always is any violinist knows, or may easily know, that the bow may travel as much swifter than the string oscillates as the player chooses, many times, when great power is required, with a velocity six or eight times that of the string, often moving a distance of even thirty and forty inches in a second, *since the greater velocity of the bow will always be sure to catch the string exactly at the commencement of each of its swings in the direction in which the bow is traveling at the time, and thus facilitate its movement from the start!*

Strange to say, the thing turns out exactly the opposite of what Professor Helmholtz supposed, and the facts are precisely the reverse of those on which his elaborate theory was based! While he tells us, as just quoted, that *the string* always and necessarily *travels slowly with the bow,* and swings back "with much greater velocity than it advanced," the same as a trip-hammer falls, it is here demonstrated to be a scientific fact, that, in all ordinary playing, the string positively travels at least four times faster with the bow than it can oscillate when released, as it is perfectly clear that it can only fly back at its normal velocity or rate of swing, in proportion to its length, size, weight, and tension. Thus, the string in all ordinary playing absolutely acts in diametrical opposition to what Professor Helmholtz teaches, since it travels with the bow, or while it clings to the rosined hair, "with much greater velocity than it" retreats, after being momentarily released, since it can only swing back in accordance with its normal pendulous rate of oscillation, or at a speed of, say, *three to six inches a second,* while it is compelled to travel with the bow or while clinging to it at the rate at least of the bow's movement, or a full average of a foot to two feet a second! It thus makes its journey with the bow in about one quarter the time it takes to return! There is not, perhaps, in the investigations of science a case on record where all the facts and figures relied on to favor a theory have been so clearly and demonstrably shown to be exactly the reverse! I challenge the world to show a parallel. Assumed facts of science have been often proved to be incorrect and entirely misapprehended; but never, so far as I know, to be precisely the reverse, in every sense of the word, and to demonstrate the exact opposite of the explicit requirements of the hypothesis, and that, too, when the theory is under the manipulation of its ablest exponent.

Another marked peculiarity of this string's movement, which this careful investigator appears never to have thought of, must not be here overlooked. While the string is traveling with the bow at a *much greater velocity* than it can swing backward, it must necessarily travel at a *uniform speed* from the commencement to the end of its journey in that direction, since the bow necessarily travels in that manner; whereas, when it retreats, after being released from the rosined hair, it at first starts back slowly, moving faster and faster, the same as a pendulum, till it reaches the center of its amplitude and accomplishes one half of its swing, from which point it moves on by its acquired momentum through the other half of its journey, swiftest as it leaves the center, but slower and slower till it reaches the other limit of its swing. No one disputes this pendulous movement of a string, when drawn aside and released. With this self-evident law before him, Professor Helmholtz tells us that the string, after being released from the rosined hair, *swings back just as a hammer falls after being released from the trip-wheel;* whereas, any school-boy who has studied natural philosophy a month knows that a hammer starts slowly at the commencement of its descent, and falls faster and faster to the end of its journey, increasing in velocity throughout the entire distance by a certain definite ratio based on its constantly accumulating momentum added to its gravity, which ratio of increased velocity would be maintained by a body falling toward the earth for any distance, if a thousand miles, minus the resistance of the air. Is it possible that this greatest of modern physicists is not aware of this law of a falling hammer, and of the pendulous law governing the movement of an oscillating string when drawn to one side and released? To suppose him ignorant of these well-known laws is to suppose an impossibility. To suppose he knowingly misrepresented the facts, to favor the theory of "vibrational form" he was laboring to establish, is inconceivable. I leave him to the mercy of a charitable world.

Such erroneous and superficial conceptions of the physics of sound generation and propagation as the foregoing, are the very kind of scientific data on which the entire wave-theory rests. Yet with all these and similar absolutely laughable misapprehensions, which will be abundantly pointed out as the argument advances, I am sincerely and kindly cautioned by my friends not to assail this theory, or venture into collision with such names as those of Tyndall, Helmholtz, and Mayer, unless I desire to be so finely pulverized, as one of them expressed it, that it would "require a microscope of several horsepower to detect the fragments!" The reader can well imagine, that, knowing as I did of scores of just such scientific escapades by these great authors, such as those I am now evolving from their writings, I I felt very little alarm at these annihilating predictions.

In view of the foregoing inversion of the facts and arguments of Professor Helmholtz, showing them to establish the exact opposite of what he intended them to prove, what must become of the various graphical diagrams which this writer has taken the trouble to prepare for his book, illustrating the "vibrational form" supposed to take place in bowed strings, every one of which is based on this idea of the trip-hammer moving up only a tenth as rapidly as it falls, and this self-evident fallacy that the bow must act in the same way, always traveling about *ten times* slower than the string's normal oscillation? A child might have confounded this great philosopher by asking what makes the string vibrate at all if the bow travels *ten times* slower than the string naturally swings? For, it is a recorded fact, that, in his very first diagram illustrating this principle of the trip-hammer's movement and that of a bowed string (page 32), he shows that it takes the hammer *ten times as long to be lifted as it does for it to fall;* whereas the intuition of the child would have taught him that as the motion of the bow causes the string to keep up its oscillation, it must of necessity travel as fast at least as the string can oscillate, and in all ordinary playing much faster! And what, I may ask, further, becomes of his "law," which he so elaborately formulates, that the *quality of tone* is caused by the vibrational form of the oscillating instrument and of the air-waves which it thus produces, when his principal graphical illustration and proof of this law, repeated five times, is this same misconception of the bow having only about one tenth the normal velocity of the string?

As I have clearly shown, by figures which every physicist will admit, and which any observer can see to be correct by the least attention to a violinist when playing, that in all ordinary execution on the violin the bow must travel and actually does travel at least four or five times as fast as a string normally oscillates at the nodal point, moving from twelve inches to two feet a second, *thus carrying the string along with it four or five times faster than it can fly back again*, it gives us the somewhat novel and startling mechanical improvement in trip-hammers which would require them to fall only about one quarter as fast as they are lifted by the mill-work, that is, if their movement corresponds to that of the string when excited by the bow, as this philosopher teaches! If his mechanical ideas concerning the principle of a trip-hammer's movement are here correctly represented by the motion of a string as compared to that of a bow, I doubt if any mill-owners would care to employ him to superintend the construction of their machinery! A trip-hammer falling with only one quarter the velocity of its ascent, as is proved to be the case with the string, would do but little forging unless the anvil were placed above it, which is evidently the way this philosopher would have to construct it! But I will not be too hard on him, and will agree to let him off on the condition that he at once renounce the wave-theory of sound and adopt the hypothesis of substantial sonorous pulses!

A true theory is always consistent with itself, or at least *may* be, even down to the unimportant minutiæ of its details; and though there may be phenomena involved in its analysis which it can not explain, such phenomena, nevertheless, can not contradict it; whereas a false theory, however plausible or apparently consistent in its principal features, is certain to contradict itself in the discussion of details. Such we shall see to be the case all the way through this investigation of the wave-theory of sound.

This fallacious reasoning of Professor Helmholtz, based, as we have seen, on his utter misconception of facts which the commonest observer should have noted, is not a whit more surprising than that of Professor Tyndall, just hinted at, in supposing that a tuning-fork's prongs must necessarily move with enormous velocity, when, in the very nature of things, as the reader can instantly calculate, they can not travel in one direction over seven or eight inches a second, or, counting both directions, more than fourteen to sixteen inches in the same time. This being the fact, what, then, becomes of his "condensations and rarefactions" of the atmosphere wrought by this snail-like motion, with the *heat* and additional *elasticity* of the air thus generated sufficient to add "*one sixth*" to the velocity of sound, which hypothesis is absolutely essential to the existence of the wave-theory, as will be soon demonstrated? I will again quote his language:—

"Imagine one of the prongs of the vibrating fork *swiftly advancing* [at the rate of *seven inches* a second!]; it *compresses the air* immediately in front of it, and when it retreats it leaves a *partial vacuum* behind, the process being repeated at every subsequent advance and retreat. The *whole function* of the tuning-fork is to *carve the air into these condensations and rarefactions*."—*Lectures on Sound*, p. 62.

The Professor may well request us to "*imagine* one of the prongs of the vibrating fork *swiftly* advancing"; for, whenever the reader is undeceived on this subject by a correct statement of its facts, and thus becomes aware that the prong of the fork only moves *seven inches* in a second, not one half as fast as a year-old baby can walk, it requires a considerable stretch of the *imagination* to see it "*swiftly* advancing," thus *carving* the air into a "condensation," and then retreating so "swiftly" as to cause a "rarefaction" by leaving a "*partial vacuum behind*," all of which generate the required heat and elasticity to enable these air-waves to travel with sufficiently augmented velocity not to contradict the wave-theory! Not a word does this scientist suggest as to the possibility of the fork generating its tone by the molecular effect of its numerous sudden stops and starts on the atomic structure of the instrument itself, the only rational supposition possible in the premises! An intellect capable of *imagining* a tuning-fork "*swiftly* advancing," and generating heat by squeezing the air into "condensations" when only traveling at the rate of *seven inches* a second, could hardly be expected to grasp an idea so beautiful, fundamental, and scientific, as the one suggested by the above molecular hypothesis.

I have sometimes wondered if this lecturer ever thought of the really amusing character of this tuning-fork's performance, as he has described it! He tells us that when it advances it "*compresses the air immediately in front of it*, and when it retreats *it leaves a partial vacuum* behind." Now, this amounts to an unprovoked scientific slander on our atmosphere! With all its acknowledged *elasticity* or spring-power, especially under *pressure*,—one of its most persistent, important, and undeniable characteristics,—it is here made out to be so *lazy* and *sluggish*, under the manipulation of this learned savant, that, even after it has been *compressed* into a *condensation*, it allows the prong of a tuning-fork when traveling but *seven inches a second* to run right away from it and leave a *partial vacuum!*

Seriously, I think it is about time for physicists to call a convention, and reconsider this entire question of sound-propagation, or else hire some good mechanic to reconstruct their wave-theory, and so to arrange it that its parts will hang together, unless they want the whole thing to become the laughing-stock of the unscientific world! For, at the present rate of progress, Professors Tyndall and Helmholtz, its two ablest and most popular exponents, are fast bringing the hypothesis into contempt. To make out, as they do, that the *compression* of the air, by this slow forward movement of the fork's prong, will send off a condensed wave 1120 feet a second, or at the observed velocity of sound, and then tell us that the same condensed wave, after being compressed, can not recoil fast enough *to keep up with the retreating prong and prevent a vacuum, requiring only this same velocity of seven inches a second*, is laughable enough to have a place in the funniest column of *Punch.*

Returning now for a moment to the tuning-fork upon its resonant case vibrating by sympathy 180 feet distant from the actuating fork, I ask what explanation can possibly be given of such a sonorous effect save the one assumed in my hypothesis of substantial pulses, having a definite law controlling their velocity of propagation? We have seen that literal, physical air-waves, moulded and driven off from the prongs of the oscillating fork, moving but seven inches in a second, if they should travel as swiftly as the moving prongs themselves (and they surely can move no faster), and if all inertia and atmospheric friction, or tendency to quiescence, were abolished, *would require over five minutes to pass from one fork to the other!* Yet we absolutely know that the sympathising fork commences responding to the other the moment the sound is heard by the assistant standing near it, or in almost one two-thousandth part of the time it would take an air-wave at its highest possible velocity to reach it were there nothing to hinder its progress!

On the hypothesis of sound consisting of substantial pulses generated by the actuating fork, augmented and diffused by its resonant case and its sympathetic air-column, and radiating through the atmosphere by a law of conduction peculiar to sonorous pulses, as light is radiated by a law peculiar to luminous discharges, it is easy comprehending that such sonorous discharges might travel to the distant fork at a velocity of 1120 feet a second, or at the observed velocity of sound, without any regard whatever to the intervening air except as to its conducting properties (the same as electricity depends on the character of its conducting medium), acting, at their arrival, first on the sensitive unison air-column which fills the resonant chamber, and which, being so exceedingly mobile, will of course first respond by sympathetic action, which is instantly communicated to the surrounding case, and, through it, to the prongs of the fork.

One of the most fatal and mischievous errors, and one which has tended, perhaps, more than any other to keep the wave-theory of sound in existence, is the assumption, that, because an inclosed air-column, a singing flame, or a stretched membrane, will stir at a distance from an actuating instrument of the same pitch *the intervening mass of air throughout the whole distance must therefore be thrown into vibratory motion.* This fallacy led to the invention of an all-pervading luminiferous *ether*, to account for, or rather provide for, the undulatory theory of light. This hypothetic ether is supposed to fill all interstellar space, the entire mass of which must, of course, be thrown into waves, and must continue perpetually to vibrate by the light of *one single star*, which, of course, shines through it in all directions; while millions of other stars also shining through the same mass in all directions must necessarily produce millions of independent co-existing and conflicting systems of waves within the same mass of ether at the same instant! Thus, taking any single cubic inch of interstellar space you choose to select, the ether which it contains must be actually oscillating with a million different systems of waves, from a million different stars, while these millions of diverse and conflicting motions of the same ether are carried on harmoniously at the same instant and without the least disturbance of each other, according to this consistent and highly scientific hypothesis of wave-motion! Yet the same authorities tell us that two systems of aerial or ethereal waves "*interfering*" will completely neutralize and destroy each other!

Having seen how a unison air-column can resound by means of synchronous but substantial pulses dashing against it, let us revert again for a moment to the sounding-board, whose principle of resonance, as before intimated, is entirely different, and try to learn how the sound of a fork is augmented by its stem simply being held in contact with the wood, if it is not caused by the augmentation of air-waves, as the undulatory theory supposes it to be.

The fundamental laws of conduction and radiation, lying at the bottom of this and all analogous phenomena, such as those of Heat, Light, Electricity, Magnetism, &c., are not understood, and probably never will be by man. It is only by the analogies of the so-called forces, elements, and modes of motion, that we can arrive at any definite or satisfactory conclusion on the subject. We definitely know, however, from the best of analogical reasons, that the resonance of a sounding-board can be nothing but the simplest radiation of sonorous substance, the same as heat is radiated in larger quantities from a more extended surface or from one of a better radiating material. No one pretends to believe that heat radiates or diffuses itself through a room from a metallic surface by means of augmented *air-waves* driven off, though the atmosphere may tremble, and no doubt does, from the effects of such radiating heat. But as some kind of an undulatory motion seemed to be necessary for heat, in order to keep up its complex analogy with sound-waves and light-waves, that "all-pervading" *ether* (which has no existence in fact, but which Professor Tyndall describes as resembling "jelly,") has recently been pressed into service, and now, instead of heat being a common-sense substance, as simple as odor or the atmosphere itself, it is converted into a certain mode of motion of this gelatinous ether, *another substance* infinitely more difficult to believe in than the substantial nature of the very thing it is intended to explain. Thus, science, "falsely so-called," instead of simplifying the problems of Nature, and bringing to light her hidden mysteries, seems to complicate and confuse every phenomenon it touches.

Suppose, for example, a cubic inch of iron at a permanently red-hot temperature, placed in the middle of a room twenty feet square, on a cold day, its effect would scarcely be sensible a short distance from it; yet, if the same quantity of iron were spread out into a sheet thin enough to cover the floor of the room, and could be kept at the same temperature, the diffusion of heat would be so intense, owing to the greater radiating surface, that no one could live in the room for a single minute. Place the same cubic inch of permanently red-hot iron in contact with a sheet of copper, and its heat would be rapidly diffused over the surface of the sheet, and from it radiated in augmented warmth throughout the room. This cubic inch of *iron* represents the tuning-fork, while the sheet of copper answers for its sounding-board. Although the heat radiates with augmented rapidity from its more extended surface, and owing to its peculiar molecular structure, yet it requires no vibratory motion of the copper whatever to cause this increased radiation. A sheet of iron in lieu of the copper would prove a poorer sounding-board for radiating the heat, because, being a poorer material for the purpose, the heat would not spread with the same facility over its surface as over that of the copper, consequently we would feel less warmth in the room.

All these facts in regard to the radiation and diffusion of heat are instructive as analogies of the radiation of sound; and, though governed by different laws in some respects, yet the general principle of the two operations is the same. On the quality of the radiator and the extent of its surface in the two phenomena depends the amount of diffusion both of sound and of heat; and in neither case does this augmentation depend in the slightest degree on the motion communicated to the radiating surfaces, and thence to the air, whatever contingent vibration either may incidentally produce.

The same law of radiation in proportion to surface holds good with reference to odor. A quantity of musk would not diffuse itself and fill a room with its peculiar fragrance as rapidly if in the form of a ball as if it were spread out over a large radiating surface; and even then the character or quality of the surface on which it was spread would have something to do with it. The warm surface of a board would radiate the fragrance with much greater intensity than a sheet of ice. The diffusive and radiative action of odor is almost exactly the same in these respects as those of sound and heat, yet no one thinks of making odor anything but substantial emissions; and I have yet to learn that either Helmholtz or Tyndall has ever gone so far in their mystification of Nature's phenomena as to attribute the diffusion and radiation of a certain fragrance to the oscillatory petaliferous tremors of the rose and honeysuckle! They, in fact, find no difficulty whatever in accepting the proposition that a substance constituted of real atoms in the form of musk can diffuse and propagate itself by an unknown law from particle to particle of the atmosphere, and thus project its rays of substantial fragrance over acres of still air in a few minutes without any kind of undulatory motion or air-waves whatever. Yet, like sound, this substantial emanation must have a suitable conducting medium or it will not travel at all. Place a grain of musk under an exhausted receiver, and no odor would radiate to fill the vacuum. So, also, place a bell within the same receiver, and cause it to strike by suitable mechanism, and no sound emerges from this region of vacuo. The sonorous atoms generated by the vibrations of the bell, as well as the odorous atoms generated by the musk, fall powerless for want of a conductor. The substantial atoms of electricity will not travel without a conducting medium, neither will those of sound or odor. Yet, evidently, they are equally substantial.

Although electric discharges are generated by the chemical action of the acids upon the zinc in the battery, and notwithstanding this chemical process may, and no doubt does, cause a degree of tremulous action among the molecules of the metal and of the liquid while generating and releasing this wondrous substantial element called electricity, yet no one would be so weak as to suppose that this tremor actually "*sends*" off these electric pulses at the enormous velocity of thousands of miles a second, much less that they are propagated by means of air-waves or wire-waves "moulded" and "carved" by this tremulous motion of the zinc or this effervescing action of the acid! No! chemists and physicists have more reason and logic when they come to treat on the generation and propagation of electric pulses, and at once concede that although the electricity is generated and liberated by the molecular tremor of the zinc and the effervescing action of the acid, yet its propagation through a wire depends on an unknown law of conduction peculiar to that particular substance, without bringing into the solution either *ethereal*, *aerial*, or *metal* undulations. Yet, whenever they change to the production of sound-pulses, which are generated by an almost similar kind of molecular tremor, and propagated by a similar unknown law, they at once become mere children in the superficiality of their logic, ignoring all ideas of the possible radiation of substantial pulses of sound by a law of conduction peculiar to that particular kind of substance the same as electric pulses travel; but, trampling under foot all analogical propriety and consistency, conclude that these sonorous discharges are literally *driven* off as air-waves, or iron-waves as the case may be, the entire distance they are propagated by the actual motion or tremor of the sounding body, though the slightest observation would have convinced them that the pulses *start with a velocity nineteen hundred times greater* than that of the movement of the instrument which is supposed to "send" them!

I now enter upon the consideration of a sonorous problem second in importance to no other question connected with the present discussion,—a question involving phenomena which are looked upon by physicists, and especially by all writers on sound, as among the most conclusive proofs that sound is propagated by means of air-waves constituted of "condensations and rarefactions."

I refer to the well-known and universally observed effects of magazine explosions in the breaking of windows at a distance,—sometimes even miles away from the source of the atmospheric concussion. As strange as it may appear to the reader, it is absolutely taken for granted by all physicists that the concussive shock or condensed atmospheric wave which crushes in windows and sometimes even houses, is the same as the sound-pulse generated at the instant of the explosion, no distinction whatever being even dreamt of between such sound and such condensed wave of air! Yes, surprising as it will appear before we get through with this examination, not one writer on sound, among these greatest scientific investigators of the world, has been able to see the least difference between the sound of such an explosion and its concussive shock, which would knock a man lifeless to the ground if standing near the magazine! That such careful thinkers should be *totally ignorant* (I use the word *ignorant* with due respect, but at the same time mean it,) of any distinction between the two phenomena, but should employ them in their descriptions of such events interchangeably, as meaning one and the same thing, is among the most startling facts connected with the investigations of modern science.

The subject is therefore of so much importance that I shall be obliged to devote several pages to its discussion, in which I propose to show, not only that all scientific writers upon this subject so far are mistaken, but that the explosions of magazines furnish one of the most conclusive and unanswerable arguments against the atmospheric wave-theory of sound which could be desired. If the advocates of the wave-hypothesis should thus be obliged to look on and see their most important weapon wrenched from their hands and fatally turned against them, surely they will begin to consider their theory as becoming hopelessly involved.

I now call attention to the fact, which appears never to have entered the minds of these astute writers, that at the explosion of a magazine thousands and possibly tens of thousands of cubic yards of gas are instantly generated and added to the air, which necessarily, without any reference to the accompanying sound at all, shove away the circumambient atmosphere in all directions; and, in doing so, naturally and unavoidably condense its particles, thus forming an intensely compressed air-wave, which is driven away at an enormous velocity, producing the agitation and concussion at a distance which break windows, as so often witnessed. These great investigators of natural phenomena have never thought of the least difference between an effect thus produced, where a mountain of gas is instantaneously added to the air, and that of a sound perhaps equally as loud caused by the clashing of two trains of cars together or the falling of a building, in which nothing is added to the bulk of the atmosphere! No, so far from making this manifest distinction, so clearly scientific, and which, as we shall soon see, explains the whole matter at the expense of the very theory it has been supposed to favor, these sound-writers speak in the most unsophisticated manner of windows being crushed in by a "sound-pulse" sent off from a magazine explosion, ignoring entirely the distinction I am here pointing out.

As an example of this childish superficiality, I will quote Professor Tyndall's innocent description of the breaking of windows at Erith. It will surely amuse the reader, if it does not instruct him:—

"The most striking example of this *inflection* of a *sonorous wave* that I have ever seen, was exhibited at Erith after the tremendous explosion of a powder magazine which occurred there in 1864. The village of Erith was some miles distant from the magazine, but in nearly all cases the windows were shattered; and it was noticeable that the *windows turned away from the origin of the explosion suffered almost as much as those which faced it.* [This effect is simply explained by the tremendous shove given to the air, causing it to compress around the buildings equally on all sides. Professor Tyndall thinks it was the "sonorous wave" which *inflected*, and doubled its two ends around the building, thus crushing the windows!] Lead sashes were employed in Erith church, and these being in some degree flexible, enabled the windows to yield to the pressure without much fracture of the glass. Every window in the church, front and back, was bent inwards. In fact, as the *sound-wave* reached the church it separated right and left, and for a moment the edifice was clasped by a *girdle of intensely compressed air.*"—*Lectures on Sound*, p. 23.

The reader observes, no doubt with some degree of surprise, that no distinction is even hinted at in the above citation between the "girdle of intensely compressed air" caused by the cubic acres of added gas, and the "sound-wave" which appeared to accompany the concussion; but, instead of this manifest discrimination, the two are used interchangeably,—the fallacy of which will now be made apparent.

First of all, I here make an announcement,—call it a prophecy, if you like,—to which I invite the attention of Professors Tyndall, Mayer, and Helmholtz, namely, that the condensed air-wave or atmospheric concussion which breaks a window at a distance from an explosion of powder, *will be found, when tested, to be altogether a different effect from the sound produced by the same explosion, and that it will also be found to travel at a different velocity, which velocity will be in proportion to the quantity of gas added and the distance the condensed wave has traveled!* If this prediction shall ever be subjected to careful scientific experiment, which can be easily done and at trifling expense, it will be found that the velocity of the concussive shock as compared to the velocity of the sound itself will bear the following relation: For a short distance from the explosion (depending on the quantity of gas added to the air) the condensed air-wave will probably travel faster than the sound by utilizing the greater spring-power of the air at the start, but at a long distance (say three or four miles) from the explosion the sound will certainly be found to reach the observer first, since the greater expansion of the condensed atmospheric shell will weaken the effect of its elastic spring and decrease the velocity of the concussive shock. While the sound-pulse (which is a separate and independent thing from the condensation of the air caused by the instantaneously added gas) has but one uniform rate of velocity from the time it starts till it reaches its maximum distance, the speed of the condensed wave of air which breaks the window will be found to be at its maximum at the start, and gradually to travel slower and slower as a larger and larger circle of atmosphere is embraced within the wave, till finally its velocity must entirely die out with its effect, not moving probably a foot a second. And while the audible sound-pulse would necessarily be limited and entirely die out within a certain distance, there is no conceivable limit to the condensed atmospheric wave but the upper boundary of the aerial ocean, as philosophy must teach us, if we take the trouble to reflect, that a single cubic yard of gas added to the air anywhere would so act on its elasticity and expansibility as to continue the displacement and motion to its upper surface,—gradually, as before observed, becoming weaker and weaker. This is clearly taught by the principle of the conservation of force, the displacement of matter, and the persistence of motion.

It is entirely different, however, in case of a sound caused by a falling tree, for example, which does not add a cubic inch to the bulk of the air, though its report moves off with the same velocity exactly as that of the sound of an explosion. The atmosphere is merely displaced by the moving tree from in front, and has only to pass around behind the trunk and fill the partial vacuum caused by its motion, thus producing by its *mobility* (which these sound-investigators seem almost entirely to ignore) an equilibrium, without probably stirring the air half a dozen rods off. For this reason, the falling of a tree or of a building produces no atmospheric concussion outside of this limited agitation, though the sound may be heard for miles away, and might prove even equal in intensity to that of an explosion. There being no large amount of gas or other elastic material added to the atmosphere by the falling tree there is no shell of "*intensely compressed air*" driven off to a distance to crush windows, which must necessarily be the case when such a body of gas is instantly generated, compelling the air which had just occupied that space to move off at great velocity in all directions. Yet, clear and simple as this exposition must be to the reader, Professor Tyndall, with all his reputed scientific penetration, was incapable of seeing it, and hence deliberately mixed up this "*girdle of intensely compressed air*," caused by the added gas, with the *sound-pulse*, which, let it be ever so intense, is not capable of stirring the lightest feather unless tuned to oscillate in unison with its own periodic pulsations.

But I do not yet propose to leave this magazine problem, clear as it is, without further elucidation. I will now give an illustration of the distinction here pointed out between a sound-pulse and an atmospheric concussion caused by the sudden addition of a large quantity of gas, which will make it so clear that a schoolboy will be able to comprehend it, though I anticipate more difficulty with physicists who are not capable of seeing any difference between an atmospheric concussion which breaks windows and the sound generated by the same explosion.

We will figure to our minds a smooth tube, say a couple of miles long, having a closely fitting piston in one end and being open at the other. It is evident, if the piston should be suddenly forced into the tube a few inches it will create a condensation of the air immediately in front of it, which, not being able to escape sidewise, will act on the air in front of that, and so on communicating the condensation from one particle of air to another till the concussion reaches the far end of the tube, where it would demonstrate itself by acting on a candle-flame or any sensitive object, whether in tune or not, such as a feather, placed at the outlet.

This sudden shove of the piston is exactly the same in principle as the sudden addition of a quantity of gas to the surrounding atmosphere by an explosion of powder or nitro-glycerine. If the piston is moved an inch into the tube, it will, in effect, add one inch to the air in the tube directly in front of the piston, which, as a matter of course, must shove the air of the tube with a force equal to the spring-power of this condensation, and will not cease with its shoving process till its effect reaches the open air at the far end of the tube, which will then, and not till then, establish an equilibrium in the general atmosphere outside of the tube, or compensate for the vacuum produced behind the piston in giving the original impulse. This vacuum is, of course, instantly and almost completely filled by the expansive tendency of the surrounding atmosphere near it, but the equilibrium can not be said to be fully re-established till the condensation within the tube has traveled the two miles and has been added to the bulk of the outside air.

Thus far it is, of course, plain sailing, and without any chance for controversy. But right here begins the confusion of physicists. They seem to think if the piston is shoved instantaneously but a single inch, thus in effect adding one inch to the air of the tube directly in front of it, that such a condensation would travel throughout the length of the tube *with the same velocity precisely as if the piston had been shoved twelve inches or twelve feet in the same instant of time*, and thus added as many inches or feet to the air of the tube instead of a single inch; though this manifestly can not be the case, because the spring-power of a twelve-inch condensation instantly generated must be vastly greater on the column of air in front, and must drive it with vastly greater velocity toward the outlet of the tube, notwithstanding the compressibility of the air, than could be effected by a spring-power of one inch. It seems to me to be so self-evident that the speed of the concussive impulse or condensed wave along the tube must bear some sort of proportion to this force of the spring or quantity of air instantly added by the movement of the piston, that it requires no argument to prove it; and I must say I fail to form a very favorable estimate of a man's philosophical or mechanical perspicacity who can not see it, or who takes the opposite view, as do our most learned savants. So far from admitting this, as I conceive, elementary principle of physics, they actually teach the principle that if the piston could be instantaneously moved a distance of fifty feet, thus compressing this quantity of air within the space of a single inch or even less (representing the condensed force of powder before its explosion), such an expansive spring-power would not shove the remainder of the air in the tube with any greater velocity than if the piston had moved but a quarter of an inch, having the very weak spring-force such a trifling condensation would have produced! This, I admit, is a serious charge to make against the greatest scientists of the age; but I will sustain it unequivocally not only from the record but by the unavoidable logic of their explanation of magazine explosions, in making them conform to the wave-theory. Let me have the reader's attention for a few moments upon this single point.

In the first place, these physicists fully justify my charge by making the condensed wave of air which is shoved away in all directions at the explosion of a magazine, *identical with the sound-pulse which the same explosion produces*, without any reference to the amount of gas added, as just quoted fully from Professor Tyndall, with which also all other writers on the subject agree. I will illustrate this. If a single barrel of powder, for example, should be exploded at the magazine, the sound would, of course, be heard, and the concussive shock felt, at the distance of a mile away. Professor Tyndall says this sound-pulse and this condensed air-wave are identical. Then, if one thousand barrels of powder, instead of a single barrel, should be exploded at the same place, causing one thousand times as much gas and spring-force to drive the air, *the concussive shock and the sound-pulse heard a mile away would still be identical*, according to this same high authority. Now, since there is no appreciable difference between the velocity of a loud and of a feeble sound, as universal observation proves, and consequently no difference between the velocity of the sounds of the two explosions just supposed, it is clear that my charge is sustained to all intents and purposes, namely, that the logic of Professor Tyndall and his collaborators on sound teaches that the velocity of a *condensed wave* caused by the sudden addition of air or gas to the atmosphere does not depend in the slightest degree on the *quantity of air or gas added*, since both quantities and their resultant condensations in the two explosions are identical with their accompanying sound-pulses, and since all sounds have but one uniform velocity in air of the same temperature! Hence, it follows, as the result of this reasoning, that, could a piston be instantaneously pushed into our supposed tube a distance of *fifty feet*, producing the same effect as if fifty feet of additional air were instantly introduced in front of the piston, it would not drive the condensed wave toward the far end of the tube with any greater velocity than if the piston were shoved *the sixteenth of an inch*, since *all condensed waves of air are identical with sound, and all sounds have the same velocity!* There can be no escape from this conclusion, grind as it may the logic of these great scientific investigators, as will soon be demonstrated by the very words of one of the foremost of their number. To attempt to modify it in the least would be at once to abandon the identity of the "sound-pulse" and the condensed air-wave sent off from a magazine explosion, and such a modification would be the simple renouncement of the entire wave-theory of sound.

I have already explained that a condensed wave in the open air, driven off by the explosion of a given quantity of powder, dynamite, or nitro-glycerine, would travel at its greatest velocity at the start, its speed becoming slower and slower the larger the circle of atmosphere embraced within the expanding condensation. Not so, however, with the condensed wave in our supposed tube. As the wave instantly generated by the motion of the piston can not expand laterally, like the condensation caused by a magazine explosion, but must continue on in the same direct course, controlled by the same limits of the sides of the tube to its far end, it must seem evident that any given condensation caused by the moving piston will travel with the same uniform velocity from one end to the other of the tube. If the added air, or, what is the same, if the movement of the piston be small, the spring-force of the condensation thus generated will be slight, and its velocity throughout the tube will be correspondingly low; but if the piston should move suddenly a larger distance the spring-force of the condensed wave and its velocity will be correspondingly increased, though in both cases the velocity will probably be uniform, or at least very nearly so, from the start to the finish.

In assuming this condensed wave of air resulting from an explosion (which is precisely the same thing as that in the tube, since the explosion of a little powder in front of the piston would produce the same effect exactly,) to be identical with the sound-pulse, as all physicists are compelled to do according to the wave-theory, they are unavoidably forced to assume, as already demonstrated, that such atmospheric condensations, whether large or small, must travel at the same uniform velocity, without any retardation by expansion in the open air, since the velocity of all sounds is exactly the same whether caused by small or large explosions. The final result of this reasoning is, either that all additions of gas to the air by the explosions of powder, whether large or small the quantity—whether a hundred pounds or a million tons—must drive the condensed wave with the same velocity, or else such condensation is not identical with the sound-pulse, since all sounds, as every one admits, *travel with the same velocity!* This logical sapping and mining of the wave-theory must inevitably result in the surrender of the citadel, as will now be seen.

The foregoing being the unperverted and undeniable logic of physicists, let us for a few minutes turn to the record. By reference to *Appleton's American Encyclopedia* and its elegantly written article on "Sound," fortunately within the reach of all students desiring to investigate the matter, Professor Mayer, the highest authority on sound in this country and called by many the Helmholtz of America, makes use of this very illustration of the tube with a movable piston at one end, and actually assumes and teaches that the velocity of the atmospheric condensation caused by a sudden shove of the piston must necessarily be the same as that of sound, or must of necessity travel 1090 feet in a second at a temperature of 32 degrees Fahrenheit, since that is the admitted velocity of sound. As surprising as it may seem to the unscientific reader, and in exact conformity to the foregoing argument, this physicist makes no distinction whatever in the velocity of the condensed wave thus generated, whether the piston is moved one inch or ten feet, so the movement is instantaneous; and consequently he points out no difference in the speed of such a wave, whether the spring-force of the condensation generated by the piston's motion be equal to a pressure of one ounce or one thousand pounds! He assumes this velocity of the condensed wave along the tube to be the same as that of sound,—nothing more and nothing less,—and hence it must be the same necessarily, whatever the spring-force employed to drive it, since the velocity of sound through this tube at any definite temperature, as already shown, is always the same!

As this writer fails to note this distinction, but rather ignores it, the same as did Professor Tyndall in reference to the magazine explosion and the destruction of the windows at Erith by a "sound-wave," I am therefore compelled, as I did in the other case, to definitely point out the law governing the transmission both of the sound and of the atmospheric condensation through this tube, and thus indicate the manifest difference between them, which science and its exponents so far have failed to do.

Let us suppose the piston to be moved instantaneously into the tube a certain distance by the blow of a hammer, which also makes a sharp report at the same time. This simultaneous sound of the blow and atmospheric wave produced by the movement of the piston might or might not travel with the same velocity toward the far end of the tube. It would, of course, depend entirely upon the distance the piston was driven by the blow of the hammer, or, in other words, upon the quantity of air (in effect) thereby added to the atmosphere of the tube. It is evident that a true distance for the piston to suddenly move by this blow might be arrived at by experiment which would furnish just enough spring-force to carry the condensed wave through the tube with a velocity equal to but not exceeding that of the sound-pulse caused by the same blow of the hammer. But it is likewise evident that a distance might be selected for the piston to move (say *one sixteenth of an inch*) which would produce so little compression of the air in front as to cause the condensed wave to lag behind, and possibly not travel one tenth as fast as the sound of the hammer. In this case, however, the condensation, as before remarked, would probably travel through the tube at a uniform velocity from end to end, though the sound would vastly outstrip it. The speed of so slight a condensation would resemble that of a condensed wave from a magazine explosion when it had nearly spent itself by expansion and rarefaction, as already explained. And, finally, it is evident that a distance could be determined for the piston to move (say ten, twenty, or forty feet,) simultaneously with the blow of the hammer, provided it could be instantaneous, which would add sufficient spring-force to carry the condensed wave with a velocity twice or even three times that of sound. Is not this simple and clear? Yet these palpable and manifest distinctions, lying at the very basis of pneumatics and acoustics, as any analytical mind must perceive, have never entered the thoughts of these great physicists. Why? The answer is plain. Simply because the universally accepted wave-theory of sound is obliged to lay down as its fundamental principle that a sound-pulse of any kind consists in and is propagated by means of a *condensation* of the air, and can only travel as such compressed atmospheric pulse. Hence, after starting out with this fallacy, it became necessary, in order to harmonize natural phenomena, to compel all kinds of atmospheric condensations to conform to this law, and thus to travel at the observed velocity of sound! As physicists were unable to separate the concussive shock of a magazine explosion from its sound-report, but must suppose the two necessarily to be one and the same thing, according to this wave-hypothesis, it is asking altogether too much of them now to distinguish between the velocity of a condensed wave in a tube and its accompanying sound derived simultaneously from the blow of a hammer! It is owing entirely to the blinding effect of this all-pervading fallacy of atmospheric sound-waves having "condensations and rarefactions," generating thereby "heat," and thus adding "one sixth" to the elasticity of the air and the velocity of sound, that we see Professor Tyndall deliberately and almost pitiably jumbling a "sound-wave" or a "sonorous pulse" with the "girdle of intensely compressed air" *which crushed in the windows at Erith!* And it is owing to the same reason that we see Professor Mayer, one of the most brilliant intellects of America, laying down his law that the velocity of a condensed wave in a tube, caused by the sudden shove of a piston, must necessarily be 1090 feet a second, or, in other words, must conform to the observed velocity of sound, without the least regard to the amount of condensation the piston produced, or the force thus brought to bear in propelling the wave!

I will now quote Professor Mayer's own words from the *Encyclopedia*, that their clearly erroneous character may be manifest to the reader:—

> "If air were *incompressible*, a motion produced at any point of its mass would *instantaneously be transmitted to every other point of the atmosphere.*"

Then, to show what he means by the *transmission of this "motion" "to* every other point of the atmosphere," he continues, without break, to use the illustration of the tube, of which I have spoken:—

> "Thus, if we imagine a tube open at one end and closed at the other by a piston that moves in the tube without friction, it is evident that if this piston were pushed into the tube a certain distance

the air would at the same time move out of the tube at the open end. [That is, on the supposition, as above, that the air was "incompressible."] But air is *compressible* and *elastic*, and after the piston has been pushed into the cylinder, a *measurable interval of time* will have elapsed before the air would move out of the open end of the tube. *This interval is the time taken by sound to travel the length of the tube.*"

He thus not only confirms what I have already said, that the condensed wave caused by pushing the piston into the tube must necessarily travel, according to the wave-theory, with the velocity of sound, whether it be accompanied by sound or not, and without any regard to the amount or force of this condensation or the distance the piston is instantaneously moved, but he also teaches the enormous and self-evident error that "if air were *incompressible* a *motion at any point of its mass* would instantaneously be transmitted *to every other point of the atmosphere*," which "motion" he immediately explains to be the absolute displacement of the entire atmosphere to the extent of the movement! This he manifestly means to teach by his illustration of the tube, out of which the air would instantly rush as the piston was pushed into the other end, supposing the air to be incompressible, and to the exact amount of the piston's movement. A more erroneous inculcation than this can not be imagined, as I will now show.

As recently remarked, he here ignores *in toto* the *mobility* of the air, and overlooks one of the plainest principles in science, that even if the atmosphere were wholly "incompressible" it still might possess extreme *mobility*, and thus compensate for any "motion," and neutralize its effect by its disturbed portion moving around the disturbing body and thus establishing an equilibrium, without the motion being transmitted more than a few inches from the center of disturbance. Instead of recognizing this elementary fact of science, he makes no reckoning of this principle of *mobility* at all, and teaches that if the air was incompressible, a fly, by moving its wings and thus stirring the atmosphere, would actually continue the same displacement "to every other point of the atmosphere," even carrying this same motion around the earth, just as the air would move out of the tube by the motion of the piston!

Now, we have just such an element as he supposes in *water*, which is practically incompressible though possessing the same *mobility* in proportion to its density as the atmosphere. Hence, if we had an inexpansible tube two miles long filled with water free from air, a piston pushed into one end would cause the water to pass out at the other end at the same time. Why? Because, in the first place, being incompressible its particles can not squeeze together; and, secondly, its *mobility* can not be made available to counteract this motion, or to compensate for the displacement, owing to its confinement by the sides of the tube. But supposing the tube were not there, and the same disturbance of the water should take place in the open ocean by pushing the same sized piston through it the same distance, this authoritative writer teaches, if his words have any meaning at all, that this motion "would instantaneously be transmitted to every other point of the" ocean, displacing every particle of its millions of cubic miles of water to the full extent, in the aggregate, of this piston movement, just as truly and literally as that the same quantity of water would be forced out of the end of the supposed tube! There is no possible escape from this conclusion, since the water is practically incompressible, and its *mobility* is not named or so much as hinted by this physicist. I doubt if he even thought of it, or he surely would have detected the fallacy of his teaching, and not have placed on record, to stand forever, such an unmitigated philosophical blunder.

And here we are compelled to note the surprising fact, that, while these writers on sound are constantly calling our attention to the "elasticity," "density," and "compressibility" of the air, and its consequent spring-power in conveying a pulse or atmospheric condensation with great velocity to a distance, they never even name the *mobility* of the air, one of its most important and persistent characteristics! Is there any meaning in this astonishing fact, or any way of accounting for such a remarkable oversight in scientific writers? I will not say it is an intentional suppression of a well-known scientific fact, but when we come to consider that should the *mobility* of the air be recognized in their arguments on wave-motion, it would in every instance overthrow the wave-theory of sound, the coincidence becomes at once startling and suggestive! When these physicists are engaged in constructing their beautiful mathematical hypothesis of a sound-pulse causing a "condensation" of the air, which generates *heat* enough to add "one sixth" to the velocity of the sound, and which, owing to the spring-power of the air resulting from its *compressibility* and *elasticity*, is driven from mass to mass of the atmosphere at a velocity of 1120 feet a second, all by the trifling aggregate movement of a tuning-fork's prongs *seven inches in a second*, they seem to shut their eyes to the fact that if the air possesses any *mobility* at all, or the least tendency to get out of the way of the advancing prong and move around behind it, the continuation of this supposed "pulse" or "condensation" a single inch beyond the travel of the prong is utterly impossible. It is therefore clearly manifest that this principle of atmospheric *mobility* or this tendency of the air to move aside as an object is passing through it, even if its density and mechanical viscosity were equal to those of mercury, completely nullifies the hypothesis of an air-pulse or condensed wave being continued a single foot in advance of any object, if even moving as swiftly as a bullet when fired from a rifle, which travels at least 2,000 times swifter than the prong of a tuning-fork! If the air did not possess the principle of *mobility*, or, in other words, could not get out of the way of a body passing through it and thus pass around behind, then the pulse must necessarily continue on in a direct line in advance of a fork's prong the same as in our supposed tube, moving at a velocity corresponding to the velocity of the impelling body, as before illustrated. But the mobility of the air, which the wave-theory wisely and necessarily ignores, alone counteracts and neutralizes this supposed tendency of a pulse or condensation to travel any distance in free air driven by a body moving through it at whatever velocity.

The fact that any physicist claiming to think or reason, knowing of the *mobility* of the air and its perfect freedom to escape sidewise when disturbed by a moving body, should have ever taught, except as a huge scientific joke, that condensed air-waves are actually driven off at a velocity of 1120 feet a second in advance of the prong of a tuning-fork moving but *seven inches in a second*, must prove a source of almost infinite amusement to scientific investigators of the not very distant future; while the very writers, I doubt not, who now advocate these infinite impossibilities will themselves be the first to laugh at their unparalleled absurdity as soon as the question is once fairly brought to their attention.

So far, then, from the position of Professor Mayer being correct in regard to the instantaneous transmission of a disturbance to all parts of an "*incompressible*" body, it turns out to be exactly the reverse; as was seen in the analysis of the motion of a violin-string, and the enormous blunder of Professor Helmholtz. If the air were really *incompressible*, while at the same time possessing *mobility*, as seen in the case of water, this very condition would prevent such transmission instead of encourage it! But with the atmosphere compressible, as we know it to be, let a movement take place in the midst of the aerial ocean, and this very principle of compressibility will permit the disturbance to extend around for some distance, as seen in the movement of a fan in a still room, into which smoke has been admitted to visualize the motion; whereas, if the air were practically "*incompressible*," as in Professor Mayer's supposition, the same as water, the disturbance would be rigidly confined to the moving body, while the *mobility* of the air would continually come into play to reestablish equilibrium.

I have thus far spoken of water as practically *incompressible*, which it is so far as any ordinary motion producing an appreciable effect is concerned, since its utmost compressibility which mechanics has been able to demonstrate, amounts to but one part in 22,000 for each atmosphere, or fifteen pounds pressure to the square inch. It is perfectly evident that the mobility of a body in no way depends upon or is related to its compressibility, since mercury is just as mobile as water, while it possesses but *one twentieth* the compressibility, or but one part in 440,000 for each atmosphere. Even the mobility of atmospheric air itself does not exceed that of quicksilver, though the air is the most compressible of all corporeal substances, since it is susceptible of reduction in bulk by pressure till it contains 770 atmospheres, when its density, which would be equivalent to its weight, would exactly equal that of water at sixty degrees Fahrenheit. We thus see that a fluid might be assumed to be absolutely *incompressible* and yet retain the highest degree of mobility, which completely annihilates the argument of Professor Mayer.

A little reflection must teach us that, if we suppose the air to be really "incompressible," a motion would have to be sufficiently powerful to displace the entire atmosphere with its millions of tons weight in order to instantaneously effect this transmission of "motion" to its extreme limits, as Professor Mayer asserts! To illustrate it, suppose the experiment to be tried with water. According to the teaching of this savant (and it is impossible for his language to be misunderstood), if a *moneron* should move its body at the bottom of the ocean, four miles below its surface, supposing the water to be incompressible, or should thrust out one of its pseudopodia, the *mobility* of the water directly around this little creature counts for nothing at all in the scientific estimation of this physicist, since he wholly ignores it; but in lieu of this, he tell us the "motion" would absolutely be "*transmitted to every other point of the*" *ocean*, or, in other words, the entire ocean would be displaced bodily to the aggregate extent of this movement, thus requiring the physical lifting force of thousands of millions of tons by the efforts of an animal no larger than a pin's head, since the weight of the entire ocean rests upon it, and being "incompressible," must be displaced to its farthest limits, according to this highest American authority on physics! A philosopher who really and deliberately supposes that if water were "incompressible," which, as we see, it is almost, he would actually stir the *entire ocean*, and thus displace its countless millions of tons by dipping his finger into it, as unmistakably taught by Professor Mayer in the quotation I have made, since the *motion would be instantaneously transmitted to every part of it*, notwithstanding the wonderful *mobility* of water and the facility with which its particles accommodate themselves to the movements of a disturbing body, can hardly be pronounced the proper man to write important scientific articles for encyclopedias. I say this with all deference to his great ability and his acknowledged scientific achievements, since it is entirely evident that the errors into which he has fallen, and which have equally misled the greatest physicists of all ages, are due to this prodigious fallacy of atmospheric wave-motion, and not to any fault as to his scientific education.

Returning to our supposed tube for a moment, and the transmission of a condensed wave through it by the motion of the piston, it is well to note the fact that Professor Mayer does not confine his unscientific reasoning to the *pushing* of the piston alone, but reverses the operation and supposes the piston to be *withdrawn* a short distance, with an exactly corresponding effect. It is undoubtedly true that this withdrawal tends to rarefy the air immediately behind the piston, and necessarily causes the entire atmosphere of the tube to move backward and fill up the vacuum thus produced. The palpable error into which he here falls, is in making the velocity of this "rarefaction" necessarily the same as that of the "condensation" caused by instantaneously pushing the piston, and both of them necessarily the same as that of sound, whereas, if he had duly considered the matter, he would have seen that while the vacuum caused by the instantaneous backward movement of the piston is *limited*, and can only produce a suction-force of about fifteen pounds to the square inch, whatever be the distance the piston may travel or whatever the length of the vacuum produced in the tube, the spring-force of the air caused by *compression* is practically unlimited, depending entirely upon the distance the piston is supposed to be instantaneously pushed forward, since atmosphere may be, as we have just seen, compressed with sufficient force to produce a spring of 1,000, 5,000, or even 10,000 pounds expansive power to the square inch. Yet this manifest difference between the maximum force of a vacuum (fifteen pounds) and the unlimited spring-force of a condensation (from one ounce up to 5,000 or 10,000 pounds), with which every student of natural philosophy is familiar, is wholly left out of the calculation by this learned physicist, the same as was the *mobility* of the atmosphere.

I again assert that it is upon this very kind of scientific (!) reasoning that the wave-theory rests; and it is these very misapprehensions about the possible velocity of the transmissions of "condensations and rarefactions" of the air, while ignoring its mobility, which have led physicists into the monstrous errors, already exposed, of the assumed propagation of air-waves at a velocity of 1120 feet a second, sent off by the aggregate movements of a tuning-fork's prong but *seven inches!* It is, in fact, these very false notions here pointed out, combined with the sheer want of a little attention, which have led all sound-investigators to detect no difference between a *condensed wave of air* caused by the addition of a large quantity of gas at an explosion and the *sound-pulse* which is simultaneously generated. Professor Tyndall, by this weak system of reasoning, as has been fully shown, necessarily supposed it was the "sound-pulse" which broke the windows at Erith, when the least attempt at philosophical analysis would have convinced him that the sound had nothing whatever to do with it, and only accompanied the "*girdle of intensely compressed air*" which did the work of destruction, the same precisely as the so-called tidal wave crushes shipping and houses when sent off by a volcanic explosion beneath the water.

It would be just as sensible and scientific for the physicist to come before an audience and attempt to explain the tidal wave which recently shattered the shipping and destroyed a town on the Pacific coast of South America by calling it an *aqueous "sound-pulse,"* as to do the same thing with the condensed air-wave which crushed the windows at Erith! The two upheavals are entirely analogous, only the one acts on the ocean of atmosphere while the other acts on the ocean of water, while they are susceptible of precisely similar solutions, since the tidal wave, as has often been observed, is accompanied by the sound of the submarine explosion, showing that this sound has nothing whatever to do with the *aqueous concussion*, as a very stupid schoolboy ought to see.

If this great scientific lecturer should ever undertake to account for the phenomena of tidal waves and their destructive effects on shipping and houses, I guarantee that he would employ no such superficial and fallacious reasoning as he did in regard to the explosion at Erith. He would at once recognize, unless I underestimate his sagacity, the proper distinction between the *rumbling sound-pulse* and the *aqueous concussion* generated and radiated by the same volcanic upheaval, and would not think of perpetrating such a stupendous scientific imposition upon his audience or upon his own intelligence as gravely teaching that the shipping and buildings were shattered by a "sound-wave" of "intensely compressed" water! I repeat that he would not think of applying to tidal waves his logic in regard to magazine explosions (though the philosophy of the two cases is precisely the same), unless his mind is more deeply imbued with the fallacies of the wave-theory of sound than would seem to be possible. Then, if this be the true explanation of tidal waves, which no one can question, Professor Tyndall has only to apply the same reasoning to the explosion, and the shattering of the windows, at Erith, and his wave-theory of sound would at once vanish into air many times thinner than one of his thinnest "rarefactions"!

It now becomes a matter of curiosity to know whether these great investigators of sound-phenomena will be able to comprehend the distinctions here so elaborately pointed out. Or will they continue on in the future, as they and their predecessors have done for centuries past, to represent the "girdle of intensely compressed air" which is driven off by a magazine explosion and which crushes in windows and even buildings, as identical with the "sound-pulse" generated by such explosion and radiated at the same time?

If they shall not yet be able to distinguish between these two distinct effects, then let them try the experiment of burning a couple of barrels of powder, and observing the effects at two separate stations, —distant, say, one and two miles,—with suitable instruments for recording the two arrivals of both the condensed wave and the sound report, and I again predict and guarantee that they will have an abundant reason for abandoning the wave-theory of sound by learning, to their amazement, that near to the explosion the concussive shock will outstrip the sound, while at a sufficient distance from it the sound will arrive some seconds in advance of the concussion.

I have thus ventured this scientific prediction in direct opposition to the universally accepted theory of sound, and in the face of the prevailing opinion of scientists in regard to the identity of the sound-pulse and the condensed atmospheric wave caused by an explosion. Should any scientific association consider this prediction of sufficient importance to waste a barrel or two of powder upon it, let them explode the former by exploding the latter; and, should they be successful in doing it, no one will feel more gratified at the result than the writer.

Directly related to the foregoing, we encounter another difficulty of similar import. Advocates of the wave-theory labor under an ever-present misconception that there is an exact similarity existing between the cause of the stirring of a unison body by sympathetic vibration (governed, as I will show, by a law of affinity as real and as impossible for us to understand as is that of magnetic attraction,) and that of the breaking of a window by this concussive atmospheric shock produced by an explosion; whereas there is a difference between the two principles, their causes, and their effects, as wide and as deep as between any other observed natural phenomena. I will here, as in the preceding case, try to point out a rational distinction.

We are referred to the fact, as a proof of this assumption, that a very thin and brittle vase may have its air-chamber so accurately tuned to the pitch of an organ-pipe that a powerful peal will cause such sympathetic vibration as to shatter it. The same thing has also occurred with panes of glass which happened to be so secured at their edges and held with such tension that a loud unison tone from the organ by sympathetic vibration has caused them to break. Yet all the air-waves ever generated by vibratory motion, if wrought in silence, I care not what their synchronism might be, could never break a vase nor stir a pane of glass by exciting sympathetic action. This self-evident distinction between atmospheric vibrations with or without accompanying tone, may be new to scientists, but it is nevertheless a distinction they are compelled to recognize.

This mysterious sympathetic action of an organ-tone on a unison body, or on a body tuned to make the same number of normal vibrations per second, by which a pane of glass may be broken by a certain organ-peal, must not be confounded with the concussive atmospheric shock caused by an explosion, as just explained, which crushes in windows indiscriminately, without the least regard to their unison tension. Writers make no distinction whatever between these effects, as just seen, but note them promiscuously as the result of atmospheric sound-waves. I offer the following single remark, which I trust will point out the difference:—

In the case of an explosion, no matter what the pitch of the tone may be, or what the vibratory tension of the thousands of panes of glass to be broken may be, such glass will be broken exactly in proportion to the force of the atmospheric wave, or the quantity of gas generated and added to the air, and the distance from the origin of the explosion. Is this not plain? Whereas in the case of the pane of glass vibrating from sympathy and breaking by a unison tone of the organ, no other tone save of that identical pitch could have affected such pane of glass in the slightest degree. If all the pipes of the organ, save that one, had been made to peal out in a single concentrated blast—even if the combined sound were of a hundred times the intensity of the one pipe referred to—they would not have stirred the pane of glass, because no sound in the combination contained the necessary synchronous pulses to cause sympathetic action. The reader, I am certain, must see the difference between these various classes of phenomena, however physicists may jumble them together in their learned essays and lectures.

Professor Tyndall gives an account of two clocks placed close together against a wall, with their pendulums so accurately adjusted in length that the *ticking* of one clock finally starts the other by sympathetic action, and of course attributes this result to the air-waves sent off by the vibrating pendulum. But to show how erroneous is this assumption, let the escapement of such actuating clock be so muffled that the pendulum will be made to move in silence, or oscillate without the music of its "ticks," (and let the clocks be so placed that their supports will not oscillate from the motion of their pendulums,) and it may run till it wears out without stirring its neighbor, notwithstanding its hypothetic air-waves, which are just as real in the one case as in the other, dash in synchronism against the pendulum to be moved.

It is a singular fact, frequently observed, that dogs will howl at the sound of a horn or other loud musical tone. Who knows but that the sonorous discharges from the instrument may act by sympathetic synchronism on the laryngeal muscles or the unison tubes of the animal's trachea, causing thereby a vibratory sensation to which he gives way in a prolonged howl? In support of this supposition, it is a fact, as observation shows, that tones from a horn about the pitch of that portion of the scale employed by the dog are more apt to excite howling than notes of a distinctly different pitch. I throw out this hint without indorsing it. Possibly a deaf dog would not be thus affected, which would indicate that the sympathetic action of the tone was conveyed to the vocal organs through the tympanic membrane, and not through direct contact with the trachea.

The hypothesis of sound as substantial emissions furnishes a beautiful explanation of the well-known phenomenon of the rising pitch of a steam-whistle as a locomotive approaches the listener, and its sudden fall as it passes and recedes.

The pitch of the whistle, as is well known, is produced by a certain number of vibrations per second, which causes, as I assume, a corresponding number of sonorous discharges to come in contact with the tympanic membrane. If the pitch of the whistle, when the engine is at rest, is the same as that of the A-string of the violin, it has 440 vibrations to the second, and consequently emits 440 pulses of sonorous substance, now supposed to be so many air-waves. The number of vibrations to the second necessary to any particular pitch is definitely ascertained by means of an instrument called the *siren* (which will be explained in the next chapter), and the following explanation is based on the known velocity of sound through the air being 1120 feet a second at ordinary temperature, or about 60° Fahrenheit.

If the whistle is sounded while the locomotive is at rest, 440 sound-pulses thus reach the ear of the distant listener each second, and consequently the pitch of the tone is A, as before observed, since it takes just that many pulses per second to create that pitch. But if the locomotive starts toward the listener at the rate of 60 miles an hour, its own speed (88 feet a second) is added to that of the sound, and consequently an equal proportion of the 440 (or about 35 more) sound-pulses strike the ear each second, which actually raise the pitch about one note in the scale, since the greater the number of sound-pulses striking the ear in a second the higher is the pitch of the tone.

But as the locomotive passes the listener at this rate of speed, the tone of the whistle is observed instantly to fall about two notes of the scale; for, in receding, it also subtracts 88 feet a second from the speed of the sound, consequently deducts another 35 sound-pulses from its pitch when at rest, making a difference of about 70 pulses between its approaching and receding tone. In a word, as the whistle when approaching causes a greater number of sound-discharges to strike the ear than when at rest its pitch is raised, so in receding it allows a lesser number to strike the ear, which correspondingly reduces the pitch.

Can any explanation of this interesting problem by means of atmospheric undulations be more simple or satisfactory, even if such air-waves had a real existence? But when it is considered that a steam-whistle can not stir the atmosphere thirty feet from the locomotive in any direction (except, as before provided, in case of sympathetic vibration), and that what aerial movements are thus incidentally produced in the immediate vicinity of the locomotive can not, by any possibility, travel at a velocity of more than four or five feet a second, less than the two hundredth part of the velocity of sound, the beauty of the new hypothesis of substantial sound-pulses, as well as its absolute necessity for solving the problem, becomes strikingly manifest, for otherwise the mystery of sound-velocity is wholly without explanation.

Another fatal misconception of scientists in regard to the laws and principles brought into play by the necessities of the wave-theory may be here pointed out. They tacitly assume—in fact their hypothesis compels them to assume—that there are two entirely distinct principles of *wave*-motion in atmosphere, or, in other words, that there must necessarily be two entirely different classes of air-waves: one suited to their sound-theory, which will travel 1120 feet a second; and another class, adapted to common sense, which will not move more than four feet a second,—both manufactured in substantially the same manner. For example, they all know and will readily admit, if I move a string or piece of wire back and forth in my hand through the air with the most perfectly pendulous regularity, and cause it to travel at an aggregate velocity even ten times greater than it is possible for it to attain when sounding, that the air-waves will not travel over four or five feet a second, if that fast, and will not be able to make headway through the dense air a dozen feet till they will entirely die out. But the moment the same string moves through the same with its two ends supported in such a manner as to *generate tone*, though with an aggregate velocity not one tenth as great, then, presto! it sends off air-waves, according to these learned physicists, which travel 1120 feet a second, or more than two hundred times as fast! Why this difference? The truth is, there can be no difference in their nature or manner of propagation, and these writers would certainly see it if they came once to reason on the question with any degree of scientific accuracy. The necessities of the wave-theory, it is true, absolutely require this distinction to be kept up, when the difference does not and can not exist. I will extend the above illustration, and make this arbitrary distinction so plain that a blind man can see it.

Suppose the same string to be fastened at its two ends to the same supports, and that it is caused to vibrate in the same manner precisely by plucking it in the middle. Now, if it happens to be so stretched as to oscillate *less than sixteen times a second* it makes no sound, and consequently the air-waves which pass off from it, since they belong to the *slow* class, can not travel more than a few inches in a second, as these writers will readily admit; but give its tuning-pin a turn, causing it to make forty or fifty vibrations in a second instead of fifteen, though moving exactly on the same principle and traveling the same aggregate distance, and instantly its air-waves, *moulded* and sent off in the same manner, start through the air at a velocity of 1120 feet a second! Can any well-balanced intellect see either consistency, sense, or science in this arbitrary and absurd distinction?

The true and only explanation of the matter is simply this. The air-waves moulded and sent off by the motions of the string are in all respects alike in the two cases, having about the same trifling velocity, not exceeding a few inches in a second. In the first instance the stops and starts are so slow that they generate nothing but air-waves, while in the second instance the changes of direction are sufficiently rapid to generate sound-pulses as well as air-waves, because the sudden stops and starts, at forty or fifty vibrations in a second, succeed each other so rapidly and produce such a molecular effect upon the atomic structure of the string as to cause the emission of that peculiar substance we call sound. While physicists utterly fail to make any kind of a satisfactory explanation of these phenomena on the theory of air-waves, but are forced to encounter two entirely distinct classes of aerial undulations,—one kind traveling seven or eight inches a second, the other kind traveling 1120 feet in the same time, yet both kinds produced exactly in the same way and by the same instrument, the new theory of substantial sonorous pulses steps forward, and in a single sentence, as above, untangles the whole problem, separating the wheat from the chaff,—sifting the sound-pulses from the incidental air-waves, —placing the whole question in an orderly and a systematic form before the reader. No physicist can fail to appreciate this eclaircissement, and yield his full consent to its truthful consistency, if in connection with it he will turn back and re-read the law of sound-generation as announced on page 93. The truth is, whenever scientific investigators shall come to understand that air-waves have nothing whatever to do with either the generation or the propagation of sound, and that they are no more an essential part of these phenomena than are the incidental waves sent off by a steamboat's wheel an essential part of the boat's forward progression, the wave-theory will at once be relegated to the limbo of exploded hypotheses, taking its place by the side of the Ptolemaic theory of astronomy, where it should have been consigned a thousand years ago.

The foregoing argument is beautifully illustrated by the blowing of a bugle-horn, which is often heard in a still night for a distance of *three miles* in all directions. The bugler may blow directly through his horn without producing tone, and exert all his lung-power and he can not stir a sensitive gas-jet twelve feet distant, while the air-waves he thus produces do not travel more than *four feet a second*, as I have repeatedly demonstrated by experiment, and as the reader will no doubt willingly admit. Yet the moment the bugler adjusts his lips to the mouthpiece in such a manner as to cause the horn and its air-column to generate tone by the proper molecular vibration, he manufactures and sends off air-waves, as the current theory teaches, with less than one fourth the lung-power he employed before, which shake the entire atmosphere into oscillations *throughout thirty-six square miles*, causing *every particle of the air* to change its position from a state of rest into "a small excursion to and fro"! He not only shakes this vast extent of atmosphere, causing every atom of it for three miles high to "*swing to and fro with the motions of pendulums*," as Professor Mayer expresses it, but he hurls these agitations at the enormous velocity of 1120 feet a second! He not only does all this, but, according to the wave-theory, he converts these thirty-six miles of atmosphere into 6,000 circular "*condensations* and *rarefactions*," the largest of which are nineteen miles in circumference, that is, supposing the tone to represent A, with 440 vibrations to the second, so compressing the condensed portions of these 6,000 waves at one and the same instant as to generate sufficient heat and elasticity to add *one sixth* to the normal velocity of the sound of his horn! This generation of heat and elasticity, the wave-theory tells us, is caused alone by the *compression* of the air-particles together, notwithstanding their *mobility* and freedom to escape pressure, requiring a physical force, even if each inch column of the atmosphere were confined within a tube and acted on by a piston, equal to *thousands of millions of tons*, as I will conclusively demonstrate, in a dozen different ways, before this chapter is concluded.

Is it possible that any physicist can be found, worthy of the name, who really believes that a man's lips adjusted in a peculiar way to the mouthpiece of a horn can actually produce such a mechanical compression of the air? I declare, upon my conscience, that I do not believe there is a sane man living, who, with these facts before him, can believe for a single moment in such a stupendous and transparent fallacy.

At this point in the discussion, I ought to say a few words in regard to the well-known phenomena of the *reflection* and *convergence* of sound, which correspond in all respects to the same action in light and heat. Physicists teach us that sound, light, and heat are all based on the same general principle of undulatory movement, and alike are simply "modes of motion," instead of the radiation of attenuated material atoms,—that they are all governed by the same law,—while the undulatory theories of light and heat are admitted on all hands to have had their origin in the universally accepted hypothesis of sound-waves. Professor Tyndall says:—

"The action of sound thus illustrated is *exactly the same as that of light and radient heat*. They, like sound, are *wave-motion*. Like sound they diffuse themselves in space, diminishing in intensity according to the same law. Like sound, also, light and radiant heat, when sent through a tube with a reflecting interior surface, may be conveyed to great distances with comparatively little loss. In fact, every experiment on the *reflection of light has its analogue in the reflection of sound*."—*Lectures on Sound*, p. 13.

There will, therefore, be no difference of opinion throughout the scientific world on the deduction I make from this citation, namely, that if the wave-theory of sound shall be unequivocally overthrown, the wave-theories of *light* and *heat* must share the same demolition, even if not one reference shall be separately made to those "modes of motion," since the latter only exist as deductions from the former. The reader will please remember this.

I now undertake to show, from the very nature of wave-motion, that there can be no such thing as *convergence*, *concentration*, *reflection*, &c., in the case of either sound, light, or heat. Should I succeed, I shall, of course, demonstrate the fallacy of this undulatory law, and thus, in another way, shatter the current hypothesis of sound.

I state, as a fact recognized by all writers on sound, that, in undulatory motion of any kind *there is no forward movement of the particles constituting the wave.* The forward movement which takes place is not that of the particles themselves which compose the wave, but the continual progressive change in the swell caused by the *succeeding local oscillations up and down of the wave-molecules.* There can be, in fact, no forward movement of any matter whatever in a wave, the apparent progressive advancement being only that of *motion* and not of *substance.* Hence, I shall assume, as I believe the philosophical judgment of the reader will bear me out in doing, that without the *forward* or *projectile motion* of some kind of *substantial atoms* there can be no *reflection,* since reflection, as every one knows, consists in the tangential rebound of a body under forward velocity, the rebound taking place in a direction corresponding to the angle of *incidence.* Professor Tyndall says:—

"The motion of the *sonorous wave* must not be confounded with the motion of the *particles* which at any moment form the wave. During the *passage of the wave every particle concerned in its transmission makes only a small excursion to and fro.* The length of this excursion is called the amplitude of the vibration."—*Lectures on Sound,* p. 44.

I have often observed the undulatory movements of a field of *flax* when in bloom, acted on by a steady wind. The waves, undulating over its blue and apparently liquid surface, are a perfect representation of the waves on the surface of a clear blue sheet of water, and occur by the rhythmically progressive sinking and successive rising of the individual stalks of flax as the breeze passes over them. Almost any field of small grain, when nearly ripe,—such as wheat, rye, or barley,—exhibits the same wave-effects by the action of the wind, as no doubt the reader has often observed.

Now, it is just as rational and philosophical to suppose that the waves on the surface of a field of *flax* can be reflected tangentially at the angle of incidence by striking the fence diagonally, as to assume the possible reflection of any other waves whatever. A moment's careful thought will convince the reader of the truth of this position. Take, for example, waves on the surface of a pond of water, which are referred to by all writers on this subject as illustrative of supposed sound-waves. I assert here that physicists are self-deceived, while unintentionally deceiving others, in claiming that such water-waves exhibit phenomena in any way resembling *reflection* or *tangential rebound,* in the proper sense of the term. Let such water-waves strike diagonally against a plain perpendicular surface, such as a ledge of rocks, and, so far from darting off in a direction corresponding to the angle of incidence and at the velocity with which they came, as is always the case with light and sound, they simply run along this barrier, recoiling slightly upon the next succeeding wave, the motion becoming thereby interrupted, broken up, and distorted into a mass of indistinguishable hillocks, the same exactly as a wave driven over a field of flax disappears after striking the fence by its recoil against the next succeeding wave.

Another fact, which utterly annihilates the hypothesis of sound-waves, the recoil which does take place, if any particular point of it is carefully watched, will be seen to re-act directly from the ledge of rock, moving away at right angles to the line of its surface, whatever may be the angle of incidence of the approaching wave! If there could be such a thing as the *reflection* of a wave, then, evidently, what little recoil there would be should change its direction after the contact, by this law of tangents conforming to the angle of incidence.

But the strongest reason against the possibility of waves reflecting—a reason which is simply unanswerable—is the fact that, in order to reflect, a wave is compelled to meet other waves of superior, or, at least, equal force and velocity, which, in the case of physical or corporeal bodies is an utter bar to any further progress! The common sense of a schoolboy must teach him that a reflecting or rebounding India-rubber ball must stop on meeting a direct ball of equal size, weight, and velocity. This illustration is at least directly applicable to air-waves and water-waves, as they are corporeal bodies, governed by the physical laws of inertia and momentum. In the case of incorporeal substances, such as the corpuscles of heat, light, sound, magnetism, electricity, and *ether* (if there be such a thing), this physical law which tends to neutralize two equal forces in case of collision does not come into play, since incorporeal atoms will collide and pass through each other without ei her being impeded in its progress, as seen in the rays from two magnets when made to cross each other's path. Now, it is simply impossible for a wave of water to recoil and retain its proper form after striking a rock, any further than to meet the first direct wave following it. The collision must, by the very laws which control the meeting of physical bodies of equal force, distort and shatter both the recoiling and the direct waves, and prevent all further symmetrical progress. Thus, in every way it can be viewed, the reflection of sounds, as in case of echoes which move off with the same *freedom* and *velocity* as the direct sounds, is thus shown to be impossible on the basis of wave-motion, according to the laws governing the movements of physical bodies.

The same effect as here described in water-waves will be found to hold good in the case of air-waves produced in a still room by the movement of a fan, especially if sufficient smoke be admitted to visualize the atmospheric movements. The waves, or, more properly, convolutions of air, will be seen to leisurely roll up against the wall of the room, not at the speed of sound but at a velocity of about four or five feet a second, then slightly recoil and mix up with the next succeeding convolutions, without the slightest semblance of true reflection, as I have frequently proved by practical experiment.

Tangential rebound, which is all there is of *reflection*, is only predicable, therefore, of the *atoms of a substance* moving forward with a certain *velocity*, being suddenly impeded by a resisting surface, as a child can fully comprehend in bounding its toy ball. Does not the reader's intelligence at once admit the truth of this law? Hence, as the particles of air or the supposed particles of ether in light-waves do not travel with the undulations at all, but merely oscillate up and down, making only "a small excursion to and fro," having no forward movement, it follows, therefore, that there is absolutely nothing to rebound or *reflect!* But if light and sound consist of real atoms, having an absolute forward velocity, or are projected with the speed of light and of sound against the reflecting surface, the tangential reflection corresponding to the angle of incidence is as natural and reasonable as that elastic balls shot from a gun against the same surface should rebound in the same manner and at the same angle. To a philosophical mind desiring only the truth, this scarcely needs elaboration.

This must not, however, be confounded with the rebounding of a jet of air or water forced from a hose-nozzle diagonally against a plain surface, for then the air and water particles have a forward velocity, which, as repeatedly taught by Professor Tyndall and others, can not be the case in wave-motion, every particle composing the wave having but a stationary and unprogressive oscillation.

The same thing, then, follows equally true of *convergence* and focal *concentration.* If a wave can not rebound tangentially for the want of forward movement in its particles, then it can not increase its intensity by focal convergence through a funnel-shaped tube, though the water may momentarily rise in the tube to the height of the wave, for convergence consists only in a succession of tangential rebounds or reflections from side to side of such a funnel, concentrating a greater number of particles into a smaller compass, and thus gathering force or intensity as the atoms approach the focal point. Is not this as clear as that reflection consists of a single rebound? It follows, therefore, as there is no velocity or forward movement to the particles of any wave, that it is utterly impossible to account for reflection or convergence of light or sound by the current theory of wave-motion, while these phenomena are beautifully consistent with my hypothesis of sonorous and luminous discharges. This ought to be self-evident to the advocates of the wave-theories of sound and light, since they teach us that the ether-particles composing the waves of light do not travel a single inch toward the earth in the whole journey of a ray from the most distant visible star. How, then, in the name of reason, could such ether-waves, with no forward movement to their particles, strike a reflecting surface and rebound off tangentially with the velocity of light?

Let it therefore be remembered, as a logical and unassailable proposition, that there can be no *rebound* where there is no *forward movement of particles;* and without rebound *tangentially,* or at the angle of incidence, there can be neither *reflection* nor *convergence.* Will any true philosopher call this proposition in question? If not, then this syllogistic consequence follows: In all sorts of wave-motion there is no forward movement of particles, as proved by the authority of Professor Tyndall in a score of passages. Without the forward movement of substantial particles there can be no rebound or tangential reflection. Hence, *reflection* or *convergence* of *sound* or *light* by means of undulations, and without the forward movement of particles, is a practical absurdity.

But how strikingly different is the aspect of this problem of convergence by means of a funnel, if sonorous pulses are viewed as substantial emissions radiated with a velocity of 1120 feet a second! And how beautifully may this funnel be supposed to gather up the scattering sound-particles, even when so sparce as to be inaudible without it, and thus convey distinct sonorous impressions to the auditory nerve! Viewing sound as composed of atoms under velocity, a little child, with sufficient judgment to watch the tangential ricochetting of his India-rubber ball, can comprehend the philosophy of convergence and concentration. The sound-particle, like the rubber ball, strikes the side of the funnel's open mouth and rebounds at an obtuse angle, leaping to the opposite side of its inner surface, every rebound bringing it nearer and nearer to the smaller end, till the sparcely scattered particles thus entering congregate at the focal point; and this is the history of all the particles entering this wide mouth, at which point they may be so few and scattered as to be insensible to audition, yet by this converging process may be so concentrated in numbers as to become distinctly audible at the focus.

By a similar convergence, through the means of a large funnel-shaped device on shipboard, a sufficient number of scattering sound-particles has been collected from the ringing of a church-bell on a coast, to be distinctly audible *one hundred miles* at sea, as recorded by Herbert Spencer in his *First Principles*, p. 183. Yet, as surprising as it may seem, this careful analytical thinker falls into the scientific rut of the wave-theory, and takes for granted that the whole atmosphere over an area two hundred miles in diameter was actually churned into "condensations and rarefactions," with a force which would have required the energy of more than *two thousand million horses*, all by the strength of one man's hand at a bell-rope! The laughable absurdity of such an idea will be made fully apparent a few pages further on, in which the most incontrovertible figures will be brought to bear against the wave-theory. When it is known, as an absolute fact, which is susceptible of easy demonstration, that the ringing of the largest bell in the world can not stir the air at a distance of *twenty feet from it*, except in case of sympathetic action in which a column of air is tuned to perfect unison, as already explained, the almost infinite fallacy of the current theory becomes apparent.

The successive rebounding of sound-particles from side to side, as shown by the converging and concentrating power of a funnel, is the same precisely as that which takes place in a smooth tube, by which a moderately voiced conversation may be carried on between two persons at its opposite ends a mile apart. Instead of the sound-particles radiating in all directions, as they do if unconfined, thus growing weaker in the exact ratio as they *scatter* and become *sparcer*, this tendency to radiation is checked by the inner surface of the tube, the different particles rebounding from side to side and thus reaching to a great distance without becoming sensibly weakened. While articulate sounds might thus be conveyed for many miles, it is a fact which the advocates of the wave-theory would do well to consider, namely, that notwithstanding such laryngeal action does not stir the air within the tube twenty feet from either end, the firing of a pistol into the mouth of such a tube would produce a distinct atmospheric *concussion* a mile distant, and even "*extinguish a lighted candle.*" This, Professor Tyndall, with his usual perspicacity, adduces as another illustration of the effect of a "sonorous wave" or "sound-pulse," without the least capability of distinguishing between an explosion which adds a body of gas to the air of the tube and the words of a person which merely disturb a small portion of its equilibrium! This unaccountable lack of discrimination in writers on sound, which has just been so fully exposed in our examination of magazine explosions and their effects, is one of the most demonstrable evidences of the superficiality and utter incompetency of modern physicists as scientific guides.

This assumption of scientists, that sound is propagated by means of *air-waves*, consisting each of a "*condensation* and a *rarefaction*," though infinitely impossible, as it will soon be shown to be, is nevertheless an essential feature of the current theory of sound, or, more properly, it is the very foundation of the hypothesis. It is conceded by Professor Helmholtz that no other kind of a wave save that consisting of a *condensation* and *rarefaction* of the air is possible in the midst of the aerial ocean, as there is no vacant space into which the atmosphere may be projected and depressed in the form of crests and furrows, as is the case with undulations on the surface of water or any other liquid body. He says:—

"The *crests* of the waves of water correspond in the waves of sound to *spherical shells where the air is condensed*, and the troughs to *shells of rarefaction*. On the free surface of the water the mass on compression can slip upwards *and so form ridges*, but in the interior of the sea of air *the mass must be condensed, as there is no unoccupied spot for its escape*."—*Sensations of Tone*, p. 14.

Frankly and flatly, then, this great authority has told us, and in unmistakable language, that without these literal "condensations and rarefactions" of the air there can be no such a thing as a sound-wave, since troughs and crests are out of the question "in the *interior of the sea of air*," "as there is no unoccupied spot for its escape," as on the surface of a body like water. The reader will please remember this important and unavoidable admission, which in the end will show beyond all question that the idea of sound traveling by means of wave-motion is a pure chimera, having not the slightest foundation in science or in fact.

It is perfectly plain, and must be so admitted by every one who takes the trouble to reflect, that if I can now show the entire impossibility and the undeniable absurdity of a "condensation and rarefaction" of the air caused by the transmission of a sound-pulse, that it necessarily shatters the whole wave-theory, leaving it without the shadow of a basis on which to rest.

To show that this statement of Professor Helmholtz is not a mere slip of the pen or one of his numerous inconsiderate remarks, such as his *trip-hammer* fiasco (see p. 95), I will now quote from Professor Tyndall a few passages to prove that he not only holds to the same idea, namely, that a sound-wave can not exist except as a "condensation and a rarefaction" of the air, but so essential and fundamental is this fact to the theory that he deliberately reiterates it in numerous places and in various forms. To quote all the passages from this writer in which he assumes this position, would be to copy nearly a quarter of his *Lectures on Sound*. I will therefore cite a sufficiently emphatic instance or two. He says:—

"With regard to the point now under consideration, you will, I trust, endeavor to form a definite image of a *wave of sound*. You ought to see mentally the air-particles when urged outwards by the explosion of our balloon *crowding closely together;* but immediately behind this *condensation* you ought to see the particles *separated more widely apart*. You ought, in short, to be able to seize the conception that a *sonorous wave consists of two portions*, in the one of which *the air is more dense*, and in the other of which *it is less dense than usual*. A *condensation* and a *rarefaction*, then, *are the two constituents of a wave of sound*."

"And here it is important to note that when I speak of vibrations, I mean complete ones; and when I speak of a *sonorous wave* I mean a *condensation and its associated rarefaction*."—*Lectures on Sound*, pp. 5, 69.

No one can ask a more concise and definite statement of an hypothesis than this, and we may thank these writers, particularly Professor Tyndall, for leaving not a lingering doubt hanging over the question as to what is meant by and what constitutes a sound-wave—

"A *condensation* and a *rarefaction*, then, are the two *constituents* of a *wave of sound*."—"When I speak of a *sonorous wave I mean a condensation and its associated rarefaction*."

But lest some of my readers should remember the unfortunate self-contradictions in which Professor Tyndall has involved himself and his theory, and thus be led to place too low an estimate upon his support of Professor Helmholtz, I will re-enforce the English physicist by the

American, as I did the German by the English. Professor Mayer (article on "Sound," *American Encyclopedia*) remarks:

> "A sonorous wave is *always* formed of two parts, *one half of air in a state of condensation, the other half of rarefied air.*"

I think the reader will now admit that I have struck the true scientific definition of a sound-wave, since the three leading physicists who have written on that subject explicitly concur, and thus mutually re-enforce each other.

The application of this definition of a sound-wave will not only be now made to the theory in question in a way which can not fail to test its value, but it will have an entirely different and unique application in the following chapter, in which the scientific reader will no doubt be deeply interested.

Before, however, making a direct application of this frank but ruinous definition to the working of the wave-theory of sound, it is necessary to look briefly at one of its unavoidable results and adjuncts, to which I have frequently had occasion to refer in the early part of this chapter, and that is the incidental generation of *heat* by the squeezing of the air-particles together which takes place in the production of these "condensations."

It is well known that if the air in a tube should be compressed or squeezed together by means of a piston, this condensation also generates heat, the temperature of the air rising exactly in proportion to the pressure applied; whereas, if the piston should be withdrawn a short distance, thus creating a suction in the tube instead of a compression, cold is developed by the rarefaction of the air. Professor Tyndall demonstrated before his audience, in one of his lectures, that by a sudden compression of the air in the tube a piece of amadou or common punk could be ignited, so intense was the heat generated by this condensation. (See *Lectures on Sound*, p. 28.)

It is a singular coincidence that not only are these "condensations" essential to the life of the wave-theory of sound, but the very *heat* they must naturally generate, if they occur at all, has quite recently become another absolute necessity to its existence. I will tell how this occurred. It was universally agreed among physicists that as sound traveled by wave-motion, its velocity, in passing through all bodies, must be in the exact ratio of their relative density and elasticity, or, in other words, it was this relation of density to elasticity which determined the velocity of sound through any medium. It so happened, however, that Newton, independently of the necessities of the wave-theory, calculated the exact relative density and elasticity of the air, which, when applied to the admitted requirements of the theory made the velocity of sound in air at the freezing temperature but 916 feet in a second, whereas the well-known observed velocity was 1090 feet, thus showing an undeniable discrepancy of 174 feet a second between the observed and the required velocity, or a deficit of about "one sixth" against the wave-hypothesis.

Now, while physicists were forced to admit Newton's calculation to be correct, on the basis of the air's known elasticity and density, the only ground upon which wave-motion, as they agreed, was possible, here was an absolute contradiction of the wave-theory by their own basis of calculation, since observation proved sound to travel 174 feet a second *faster* than waves could travel in an element thus constituted. What was to be done? No one thought of abandoning the wave-theory. Such a radical and revolutionary idea was impossible, since no other supposition had ever been suggested than wave-motion, and there was no one to propose this beautiful hypothesis of substantial sonorous discharges to take its place, which so completely, as we have seen and as we shall see, solves all the problems and mysteries which can be brought to bear. No one disputed or could dispute Newton's calculation, and there the matter stood, while various suggestions were made by physicists from time to time with a view to overcoming and reconciling this discrepancy.

Fortunately for the wave-theory (and the only thing which could have given it a lease of life), an idea occurred to Laplace, the great French mathematician,—if not a *red-hot* idea, at least one sufficiently *warm* to meet the present emergencies of the case. It consisted in simply utilizing the imaginary incidental *heat* generated by these supposititious *condensations* produced by these hypothetic sound-waves! An elaborate statement of this calculation of Laplace is given in Professor Tyndall's *Lectures on Sound* at about the 30th page, which only goes to show to what extent a fallacy of the most glaring and transparent nature may be bolstered up by a profound theorist, even when no foundation whatever exists for the ingenious explanation. I can not quote this long mathematical exposition, occupying some eight or ten pages, and it is unnecessary to do so, as the substance of it can be given in a few sentences. It is substantially as follows:—

If a sound-pulse really produces a condensation and rarefaction of the air, which at that time was admitted by all physicists, then it follows that the air-particles must be alternately driven out of their normal position into the condensed or heated portion of the wave, and drawn back again into the rarefied or cooled portion as each wave passes, thus causing them to keep up a continuous "excursion to and fro" as long as the sound lasts. (The reader will turn to page 78, and read extracts Nos. 2 and 3.) Now, as observation proves that sound travels faster in heated air than in cold, and as heat also adds to the elasticity of this compressed portion of the wave, it was calculated that this excursion of the air-molecules into the heated or condensed part and out again would be executed more rapidly than if no heat or augmentation of elasticity was generated, and hence it was concluded that the velocity of a given sound would be sufficiently increased by this change of temperature to make up the required 174 feet a second, or the deficiency proved by Newton to exist between the observed velocity and that which it ought to be according to the known density and elasticity of the air. Professor Tyndall generalizes it in these words:—

"The velocity of sound in air depends on the *elasticity* of the air in relation to its *density*. The greater the *elasticity* the *swifter* is the propagation; the greater the *density*, the *slower* is the propagation."—"Over and above, then, the elasticity involved in Newton's calculation, we have *an additional elasticity* due to the *changes of temperature produced by the passage of sound itself*."—"This *change of temperature, produced by the passage of the sound-wave itself*, virtually *augments the elasticity of the air* and *makes the velocity of sound about one sixth greater than it would be if there were no change of temperature*."—*Lectures on Sound*, pp. 29, 45, 46.

With this statement of the hypothesis and this assumed explanation of the discrepancy demonstrated by Newton, let us proceed at once to make an application of the data thus collected to the wave-theory in general.

I have already repeatedly shown the impossibility of a tuning-fork's prong sending off a condensed air-wave at the enormous velocity of sound by its slow aggregate movement of only *seven inches in a second,* owing to the extreme *mobility* of the air, an attribute which sound-theorists never name when descanting upon the other characteristics of the atmosphere, namely, its *density*, *elasticity*, and *compressibility*. I defy the reader or any other man to put his finger on a single passage in the writings of ancient or modern physicists where the *mobility* of the air is named or in any way referred to in connection with these hypothetic "condensations and rarefactions." No writer on sound would think of embarrassing and even smothering his theory of wave-motion by such a stultifying and laughable inconsistency, since the two things placed in juxtaposition would instantly neutralize each other by exposing the hollowness of the whole assumption, and thus furnish demonstrative proof that the slow movement of a tuning-fork's prong could not drive a wave or condensed pulse of air even a single inch in advance of it with the atmosphere as mobile and perfectly free to turn aside and take its place behind the prong as it is known to be! Hence, the policy and wisdom in these great scientific writers suppressing (I do not charge *intentionally*) all mention of this well-known principle of atmospheric *mobility* when treating on the possibility of a condensation and rarefaction being driven off 1120 feet by a diminutive body like a tuning-fork *moving through the air a distance of only seven inches!* Were there no other reasons which could be urged against this hypothesis, that sound consists alone of condensations and rarefactions of the air which are capable of generating heat and cold, the facts just stated would be all-sufficient to show the foundationless character of the supposition.

I have before intimated that one of the chief errors into which writers on sound have fallen is this superficial habit of making no distinction whatever in the effects of bodies moving *swiftly* or *slowly* through the air. The misapprehensions of Professors Tyndall and Helmholtz in supposing the prong of a tuning-fork "swiftly advancing" when it was actually moving but *seven or eight inches in a second,* and in supposing a pendulum moving "slowly" as contrasted with the motion of a tuning-fork's prong, when it was really traveling *four times* as fast, have been already distinctly pointed out. On this erroneous conception alone rests the prevalent fallacy of a vibrating string or fork sending off air-waves, with "condensations and rarefactions," at the velocity of sound, while no matter what the velocity of the fork or string might be, moving but the small fraction of an inch in one direction and then reversing the movement, the *mobility* of the atmosphere would prevent such aerial disturbances from traveling more than a few inches from the vibrating body before an equilibrium would be established and all wave-motion of the air would cease. If these two principles of the mobility of the air and the small velocity of a vibrating string or fork had ever been duly considered by physicists, the wave-theory of sound would long ago have exploded, and would now be looked upon as an error of the most glaring and superficial character.

But while I thus emphasize the *mobility* of the air, and the impossibility of a slow movement, such as that of a fork or string, producing any such effect on the atmosphere as the wave-hypothesis requires, I do not ignore the fact that a body passing through the air under very high velocity meets with great resistance. This consideration alone would prevent *condensed waves* from traveling through the air at the rate of a thousand feet a second by some trifling vibratory motion like that of a string or fork, or anything in fact short of a magazine explosion or something of equally tremendous power. No other argument would seem to be necessary to show that sound must be a substantial emission of some kind, since a physical wave of condensed air, to travel at such a velocity, must require hundreds if not thousands of tons of propulsive power to start it and then keep up the motion. How pitiably absurd, then, to talk of such condensed waves being sent off at such velocity by the infinitesimal strength of an insect!

Notwithstanding, then, the mobility of the air, it may, at the same time, present a resistibility *equal to that of a granite rock*, if the movement against it be of sufficient velocity. Meteoric stones, in passing into the upper or rarer stratum of our atmosphere, move with such velocity that they are first heated to incandescence, and in reaching the more dense portion of the air they are often crushed to atoms by the contact, scattering their fragmentary scintillations in all directions. It is only when meteorites enter our atmosphere in the same or partially the same direction that the earth is traveling around the sun, or its surface revolving, that they can reach the ground without being crushed. The hardest specimen of meteoric iron would crumble to powder on the first contact with our atmosphere should the collision take place in opposition to the earth's rotation around the sun, and thus meet a counter velocity to its own of *nineteen* miles a second; though it is easily conceivable that a meteorite might enter the air in a direction corresponding to the earth's rotation both on its axis and around the sun, and that the combined velocities might thus so nearly agree that the visitor would reach the ground at a speed which would not mar a block of ordinary sandstone. Specimens of such meteoric rock have often been found almost intact.

This mechanical viscosity of the air—that is, its tendency to resist displacement by a body passing through it—is beautifully illustrated by the fact that a mass of common gunpowder, exploded upon the face of a granite rock, will not mar it the slightest, for the reason that its conversion into gas, as well as the molecular expansion of the gas when generated, is so slow, comparatively, that the air has time to move out of the way without the rock being affected. I have even seen a man explode a pistol-charge of powder in his naked hand without suffering any injurious effect from it. But let a body of nitroglycerine of any size be placed on the flat surface of a rock and exploded, and the surface will be found to have been shattered to a considerable depth, which can only be accounted for by the rigidity of the air in resisting the enormous expansive velocity of the gas. To say that the air is as solid as a rock would seem ridiculous, yet it has a good deal of truth in it when the motion which attempts its displacement has a sufficiently high velocity.

But I have evidence to present against the hypothesis of sound-waves and their constituent "condensations and rarefactions," compared to which the foregoing unanswerable considerations are but as the softest zephyr contrasted with the devastating cyclone. I now proceed to present a single argument, which, in its ramifications and various phases, will form an avalanche of testimony against the theory so overwhelming that its strongest advocates will be forced to recognize it as entirely unassailable.

There is a well-known insect—one of the locustidæ (a saltatorial family of the order of orthoptera)—whose stridulation can be easily heard a distance of more than a mile. In the summer of 1867 I had the pleasure of listening to one of these insects singing in a grove of trees on the opposite side of a valley more than a mile wide, and it was a source of astonishment that so diminutive an insect—weighing less than a quarter of a pennyweight—could fill, as it did, four square miles, including, no doubt, a mile high, with its wonderful music! Yet such was the fact, which is well recognized by our greatest naturalists, including Mr. Darwin, who describes the same species of locust in his work on the *Variations of Animals and Plants*, and admits that its stridulation can often be heard a mile.

According to the wave-theory of sound, which I have the honor of opposing, this trifling insect, by simply rasping its legs across the nervures of its wings (for this is the way its tone is produced) creates a physical agitation and displacement of the air which converts the whole four cubic miles of atmosphere into waves, each wave consisting of two parts, a "condensation and a rarefaction," the compressed portion of which contains a sufficient augmentation of heat above the normal heat of the atmosphere, to add "one sixth" to the elasticity of the air and the velocity of sound! I unequivocally assert that no sane mind can accept such a proposition or intelligently believe it, and that any man who pretends to believe it (as all advocates of the current sound-theory must do) is self-deceived, having never seriously thought of the infinitely impossible consequences involved. I will now try to undeceive these astute physicists by pointing out the consequences, and thus prick the most stupendous scientific bubble ever inflated by man.

Within these four square miles which are filled by the sound of this insect, there are, in round numbers, 16,000,000,000 square-inch columns of air, each exerting a pressure on the earth and in all directions of fifteen pounds, or, in the aggregate, 120,000,000 tons. Now, since sound can *only travel* by means of air-waves, and as air-waves can *be constituted only* of "condensations and rarefactions," and as a condensation can *only take place* by the particles of air, as Professor Tyndall says, "*crowding closely together*," or a rarefaction occur except by the particles of air separating "*more widely apart*," and as every particle of air constituting a sound-wave, according to the same high authority, must necessarily make "a small excursion to and fro" every time a wave passes (see extract No. 3, page 78), it inevitably follows, if this theory be true, that this insect by simply moving its legs displaces all the particles of air constituting these 16,000,000,000 inch-columns for a mile high and restores them to their place again 440 times each second (its tone being very nearly A, or that of the second string of the violin), and continues this process of thus churning the atmosphere into condensations and rarefactions a full minute at a time! Do these advocates of the wave-theory really believe this? Theoretically and superficially, they may. Intelligently, they do not. Whether they do or not, however, it matters little to me, so long as their theory unequivocally teaches it, for I am not dealing with them at all save so far as they are identified with their theory.

No one will pretend to doubt, who admits the truth of the wave-theory, or, in fact, any theory involving the motion of the air by the passage of sound, but that the stridulation of this locust must absolutely displace and cause to move "to and fro" every particle of air 440 times a second throughout these four cubic miles of atmosphere, since it is manifest that there is not an inch of space anywhere within this vast area wherein the sound would not be heard if an ear were present; while no one will think of questioning the physical fact that it must necessarily require an appreciable amount of mechanical force and energy to shake a single inch-column of air for a mile high, displacing all its atoms for a certain distance (I care not how small that distance, if it is but the breadth of a hair), and then restoring them the same number of times each second.

As every particle of air constituting a single inch-column for a mile high is thus continuously shaken while the sound lasts, being alternately *condensed* and then *rarefied*, *heated* and then *cooled* (as sound, remember, can not travel without this), will some modern Laplace or Newton please figure out this mathematical problem, and tell me the exact—or, if that is impossible, the approximate—mechanical force it would require to produce this physical tremor and this continuous agitation of this column of air? I have not a doubt but that Professor Helmholtz could do it to the thousandth part of a grain, if he should set himself about it; and provided, first of all, that he could tear himself loose long enough from the ridiculous theory of sound-waves.

In order to form an approximate idea, I employed two different mathematicians to determine the problem for me, but I am not sure of their competency, since their calculations differed so widely from each other,—one of them estimating it to cost the expenditure of fifteen pounds of mechanical force per second, while the other made it about forty, that is, supposing the distance the air-particles oscillated back and forth to be the one thousandth part of an inch in amplitude. The latter gentleman, however, took into consideration the mechanical equivalent of the heat generated in the agitation of this inch-column of air, according to the calculation of Laplace, estimating such heat as sufficient to add one sixth to the velocity of sound, while the former rejected the heat hypothesis entirely, claiming that by no conceivable possibility could this column of air be changed from heat to cold, however slight the transition, 440 times a second, *or even ten times*, since it would necessarily take an appreciable length of time for the heat to radiate or be transferred from the hot part of the wave to the cold, even if such heat and cold exist, as the wave-theory requires. This suggestion, which had never occurred to me before, became at once another conclusive evidence of the infinite impracticability of the wave-theory, which actually requires the same particles of air, through which the sound, for example, of the high D of the piccolo flute passes, to be alternately heated and cooled off 4,752 times each second, since that many separate air-waves are sent off by this tone, a thing so transcendently improbable and inconceivable that it alone ought to cause the rejection of the wave-theory with any mind capable of reasoning on a scientific subject!

This view is also tacitly admitted by Professor Tyndall, since he distinctly tells us on page 36 of *Lectures on Sound* that the air is practically devoid of "radiative power." If atmosphere can not *radiate* its heat, how then in the name of philosophy can the same mass of air-particles become alternately heated and cooled thousands of times each second, as they must do according to the wave-theory? The same air-particles precisely have to become *condensed* and then *rarefied*, *heated* and then *cooled*, at this rapid alternation;

yet this "highest living authority," as Professor Youmans calls him, teaches "*the practical absence of radiative power in atmospheric air.*" If there is no power in air-particles to *radiate* their heat, and thus transfer it to other bodies or other air-particles, then it manifestly follows that particles of air once heated must continue to retain their heat, and can not continuously alternate from heat to cold thousands of times a second. Yet this "highest living authority" can not see that this "practical absence of radiative power in atmospheric air" utterly annihilates the wave-theory, which depends alone for its existence upon this almost infinite facility of change from heat to cold by "radiative power"!

Finally, to provide against the contingency of a possible excess of physical force in this calculation, I reduced the actual *vis viva* required to produce the rapid vibratory motion of a single inch-column of air for a mile high to *one pound a second*, evidently much below the actual force it would take, which reveals the tantalizing fact, as it must be to Professor Tyndall, that an insect which could not stir a half-ounce weight by exercising all its strength to the best advantage is made by the wave-theory to produce a physical and mechanical effect by the movement of its legs equal to *sixteen thousand million pounds*, as there are that many inch-columns of air to be thus thrown into violent tremor by this stridulation, as certain as there is the least basis of truth in the current theory of sound! Is it possible that any well-balanced intellect can really subscribe to this inevitable result of the theory? I care not how much this calculation is reduced in reason below these figures,—even if we suppose it to require but the *one thousandth part of an ounce* of mechanical force to shake this inch-column of air for a mile high, it would still require a physical moving power to be exerted by this locust, as any one can demonstrate by a few figures, of *one million pounds!* Is a theory requiring such manifestly impossible results worthy of the nineteenth century? Is it not, rather, utterly inconceivable that any physicist in his senses can believe, as does Professor Mayer, that these four cubic miles of atmosphere, with a mechanical pressure of 120,000,000 tons, are actually churned into condensations and rarefactions, and its particles made to oscillate "*to and fro with the motions of pendulums*," as he expresses it, by an insect which has not strength enough to compress a single cubic inch of air, if acted on in a tube without friction, *the one four hundred and eightieth of an inch*, estimating its shoving power against the piston at half an ounce? Is it possible that any man capable of reasoning at all can believe that by the motions of this insect's legs—no larger than small pins, and not exceeding in the aggregate a distance of *three inches in a second*,—air-waves constituted of "condensations and rarefactions" are actually hurled throughout this vast area at a velocity *four thousand times greater than that of the instrument which gives them their impetus!*

It will not do for physicists to "Pooh! Pooh!" this calculation, and try to blot out the difficulty or the danger to their theory by shutting their own eyes to its overwhelming character,—as the ostrich shuts out the danger of the hunter by thrusting its head into the sand,—and say, as some of them have done, "Oh, these figures are all very easily made, and look very formidable on paper, but they amount to nothing when arrayed against the long-established scientific data upon which the current sound-theory rests!" Well, we shall see, a little further on, whether or not a theory can stand on the strength of its venerable character, after being proved in a hundred different ways to contravene the unchangeable laws of mathematics and mechanics, while at the same time contradicting observation and the reason of all reflecting minds. We shall further see whether a theory can continue to prevail and rank as scientific, when its ablest advocates can not advance an argument in its support which will not, when fairly analyzed, overthrow it, as recently seen with magazine explosions and their effects in the breaking of windows at a distance. Let us now look at some of these self-annihilating efforts of physicists in support of the current theory of sound, as exemplified by the stridulation of this locust.

Writers on sound seem to keep up a show of respect for the physical laws of mechanics and mathematics, even when their premises completely overthrow their theory. While insisting on the hypothesis that sound in passing through the air produces actual "condensations" and rarefactions," which alternately generate heat and cold enough to add "one sixth" to the velocity of sound, they are unavoidably at times driven into the terrible necessity of the perpetration of *figures*, which, when analytically considered, absolutely annihilate wave-motion. In opposing the undulatory theory of sound, therefore, I do not need to put forward a basis of my own as to the physical force a tone must exert on the air through which it passes, and thus determine the corporeal strength of a locust in churning four cubic miles of atmosphere into "condensations and rarefactions." I have simply to take the figures furnished ready to my hand by these authoritative writers, and apply them to the observed sound of the locust, in order to exhibit the wave-hypothesis as one of the most inexcusable fallacies ever conceived by a human intellect.

For example, Professor Mayer, the highest American authority on sound, has not left us to flounder in the dark on this question, but tells us in explicit terms how much "compression" a sound-wave produces on the air in passing through it, so that we may have a definite basis for calculating the mechanical strength of the locust. He says:—

"This *compression* gives for the *compressed half of the wave an increase of* $\frac{1}{879}$ *to the ordinary density of the atmosphere*."—*Article on* "*Sound*," *American Encyclopedia*.

He here refers to the note C, having 250 vibrations to the second. He does not say whether a tone lower or higher than this would or would not produce a greater "compression" of the air; but we would naturally infer that the note A, with 440 waves a second, should generate more compression and a greater quantity of heat than one giving to the air-particles a less number of pendulous movements. However this may be, the difference is not essential to my argument should it be a little one way or the other, so we will consider the amount of "compression" produced by any sound to be practically the same, and assume that the figures here announced by Professor Mayer are properly and accurately calculated, with the wave-theory as a basis, which will enable us at once to determine the mechanical force exerted by any sounding body in converting four cubic miles of atmosphere into "condensations and rarefactions."

Now, as this sound, in passing through the air, actually produces such a condensation as makes the "density" of the *compressed half of the wave* "$\frac{1}{879}$" greater than that of the normal air through which no sound is passing, and since one half of the four cubic miles of atmosphere permeated by this stridulation is continually in a state of "compression" while the sound lasts, it mathematically follows that each cubic inch of air within this compressed portion—or, in other words, one half of all the cubic inches constituting this mass of atmosphere—is absolutely increased in "density" "$\frac{1}{873}$," while the other half of the atmosphere constituting the "rarefactions" is reduced in "density" in like proportion.

There is no escape from this astounding conclusion, as these are the figures of the foremost advocate of the wave-theory of sound in this country—not mine, while they are figures which the physicists of the whole world are forced to admit, since without exception writers on sound assume the same "condensations" of the air by the passage of sonorous waves which he does, and boldly claim that they generate sufficient heat by *compression* to add "one sixth" to the velocity of sound, while Professor Mayer is but the frank, outspoken mathematician, who formulates their calculations, and gives us the result in plain vulgar fractions, thus showing us exactly how much a sonorous wave must necessarily compress the air.

The culmination, then, of this destructive argument, amounts to this: As a cubic inch of air, when compressed to *double* the normal density of the atmosphere, requires a squeezing force of *fifteen pounds*, as every student of philosophy knows, it will of course take but the simplest mathematical talent to calculate the whole amount of pressure exerted by the locust throughout the four cubic miles,—since it must be the $\frac{1}{873}$ of 15 pounds to each cubic inch in the "compressed half" of this mass of air! As there are, in round numbers, but correct figures, 1,000,000,000,000,000 cubic inches within these four cubic miles, one half of which (500,000,000,000,000) is under pressure, having an increased *density* equal to $\frac{1}{873}$ of 15 pounds for each cubic inch, we reach the definite and authoritative result of 10,000,000,000,000 pounds physical pressure, or an actual mechanical energy exerted by this insect in producing its stridulation of *five thousand million tons!*

Will physicists "Pooh! Pooh!" these figures, as having no *weight* against the venerable wave-theory of sound? If they do, then they scout their own data, deliberately formulated and placed on record by one of their ablest collaborators. Any schoolboy can take the statement of Professor Mayer, quoted above, and in fifteen minutes reach the same incontrovertible result here given.

It now becomes a matter of curiosity and exciting interest to the scientific as well as to the unscientific world to know what physicists can say to these mathematical demonstrations! Will they say anything?—or will they attempt to pass the whole matter over in silence, on the ground that the writer of this monograph happens to be unknown,—having not the prestige of a great scientific reputation by which to herald his discoveries and announcements? We shall patiently wait and see. One thing is certain, whatever physicists may do or say: it now stands upon record, and will so stand while books are read, that if the wave-theory of sound be true, as presented in all scientific works on the subject, a mere insect, by the movements of its delicate legs, can and does absolutely convert four cubic miles of atmosphere into "condensations and rarefactions," exerting a literal, physical, and mechanical energy, as above demonstrated, of 5,000,000,000 tons! As such a result is an infinite impossibility, the wave-theory, without another argument against it, is thus demonstrated to be an infinite absurdity.

No doubt the reader by this time is

ready to ask: "Though you have used the stridulation of the locust to make the wave-theory of sound appear almost infinitely ridiculous, have you not also by the same illustration succeeded in making your own hypothesis of substantial emissions equally absurd? Is it possible," he might naturally continue, "that such a diminutive insect can fill four square miles with any conceivable substance, how much soever attenuated, keep up these discharges for hours, and still not appreciably diminish its weight?"

I admit the legitimacy and fairness of this inquiry, provided the one who makes it is not a believer in the hypothetic luminiferous *ether*, believed in by all advocates of the wave-theory of sound, which circulates freely in the substance of the diamond, yet is a *material substance* resembling a "jelly"! * No scientist who holds to the undulatory theory of light and this gelatinous *ether* has any business to put a question involving a doubt as to the possible tenuity or penetrability of any substantial entity, even if a quantity the size of a pin's head should be claimed as sufficient, when spread out, to cover the whole earth; but the unscientific reader has a legitimate right to ask this question, and to him I propose to give a brief, and, I trust, satisfactory answer.

I have in the preceding chapters had occasion to refer frequently to the wonderful nature and inconceivable tenuity of *odor*, though perfectly cognizable by the olfactory nerves, just as sound is cognizable by the auditory organs.

Fortunately for my hypothesis of sound as substantial emissions, I am left uninvolved in any absurdity, as I will show, by the universal admission of science that fragrance is a real corporeal substance, having definite material atoms,—so I am relieved of the necessity of all argument on that point.

Though odor is governed by a different law of radiation and conduction from those of sound, light, heat, magnetism, electricity, &c., each having its own peculiar conditions of diffusion and conduction, yet it is a probable fact, sufficiently well attested by approximate experiments, that a quantity of musk no larger than a locust, if properly distributed and with suitable conditions for confining its emanations, would fill four cubic miles with its material corpuscles, till a sensitive olfactory at any square inch of this area would detect its presence, yet if the original mass were to be afterward weighed with the most sensitive balance it would show no appreciable reduction in weight.

To add to the force of this illustration, I will adduce a well-known fact which can not fail to show the marvelous tenuity of odor, defying absolutely all efforts of the imagination to conceive it as composed of separate substantial atoms.

A *hound* of a certain breed, with highly sensitive olfactories, will follow the direction of a *fox* over hill and dale, through forest and jungle, hours after it has passed, and even when it has reached a score of miles ahead. Yet the hound does not depend on touching the tracks of the fox with his nose, or even of following its exact

* "The luminiferous *ether* has definite mechanical properties. It is almost *infinitely more attenuated than any known gas*, but its properties are those of a *solid* rather than those of a gas. It resembles *jelly* rather than air."

"To account for the enormous velocity of propagation in the case of light, the *substance* which transmits it is assumed to be of both extreme elasticity and extreme tenuity. This *substance* is called the luminiferous *ether*. It fills *all space;* it surrounds the *atoms of bodies*. . . . The molecules of luminous bodies are in a state of *vibration*. The vibrations are taken up by the *ether* and *transmitted through it in waves*."—TYNDALL *on Light*, pp. 57, 60.

path; but, as observed by the writer (having seen a fox pass hours before, and noting the exact path taken by its feet), will frequently vary rods from the true path, yet, keeping on in the general direction, will pursue his game with unerring certainty.

So defined and substantial are the odorous particles emanating from the footfalls of the fox, that a dog, on striking a trail hours old, will almost instantly decide, by the arrangement of the atoms in the air, the direction it has taken; but if momentarily mistaking the back-track, the difference, probably, in the intensity of the surcharged air warns him of his error, and leads him to reverse his course.

Before stopping to quibble about the impossibility of sound being substantial emanations from its inconceivable tenuity, let us try to grasp the marvelous lesson taught by this fox and hound. Though the wind may blow across the trail, carrying off for hours the odorous clouds which have risen from the instantaneous impress of the feet upon the earth, filling thus, perhaps, vast areas along the trail with those magical atoms of perfume, exceeding possibly in extent many times the four square miles of air surcharged by the locust, yet sufficient odor remains, extending for rods on both sides of the trail, to enable the hound to pursue his distant game with infallible precision.

I now ask the puzzled reader, who fails to see how the locust can fill an area two miles square with sonorous substance and not appreciably reduce its weight, to tell me approximately how much *reynard* has reduced his feet in size and weight by the clouds of odor diffused along his track for a hundred miles? Though the feet may have deteriorated by the roughness of the journey and their two hundred thousand impacts upon the hard earth, yet I venture the suggestion that the cubic miles of odorous substance which encompassed the trail and guided the hound, did not diminish the weight of either foot an appreciable fraction of a grain. Yet those miles of odor-surcharged atmosphere were filled with *substantial emissions*, as all science unites in assuring us, though not so tenuous, probably, as sonorous substance, yet sufficiently near it to cause the imagination to retire discomfited and confounded.

The reader thus has a rational answer to his question in this somewhat analogous substance of odor, showing that it is not at all among the impossibilities, nor is it even improbable, that the locust should fill such an area with sonorous substance, from this analogue in the fox's feet,—whilst not the shadow of an answer can be offered by the advocates of the wave-theory of sound for the reasonableness of corporeal results equal to the mechanical energy of a *million locomotives* ascribed to the physical strength of a single insect.

The possibility of a locust filling four cubic miles with some kind of tenuous substance, is not, therefore, at all inconceivable, since we have the positive demonstration that there is no imaginable limit to the tenuity of substantial emissions, as seen with odor. This fact of unlimited tenuity is a very different thing, however, from the unlimited strength of an insect in accomplishing physical and mechanical results by doing absolute work in the agitation and displacement of a corporeal body like atmosphere,—exerting an energy, as it must do according to the wave-theory, as just seen, of 5,000,000,000 tons. While the tenuity of substantial emanations is practically unlimited, so far as human intellect can conceive, physical and mechanical results, such as compressing the air or overcoming the inertia of bodies, changing them from a state of rest to a state of motion, are definitely and determinately limited and bounded by the strength of the being or motor employed! As well might we suppose it possible for a man to knock into fragments a range of mountains and scatter the particles over miles of territory by a single blow of his hand as to believe it possible for an insect to perform the work ascribed to it by the advocates of the wave-theory.

It is only our intense ignorance of the inscrutable *tenuity* and incommensurable *penetrability* of the intangible substances of Nature everywhere around us, and even within us, which could persist in causing such inquiries as the one just answered. When we come to accept Nature's unsolvable mysteries—among them her recondite and intangible though substantial entities, such as sound, light, heat, &c.—with less of scientific egotism and more of that wholesome faith in the rational hypothesis of an intelligent First Cause, the world will not be so apt to continue for centuries hugging to its embrace, under the name of "science," such a stupendous philosophical monstrosity, and, at the same time, such a pitiable fallacy as this Undulatory Theory of Sound; but with expanded freedom of thought to look into, or at least toward, the Unknowable Essence, and to conceive Him as manifested in His works,—with less of veneration for scientific formulas and with moderated respect for canonized authority in theoretical science, we might reasonably expect in the near future to solve mysteries as profound as a planetary ellipse, and overthrow scientific theories as well established as those of sound, light, and heat.

But I have not yet dismissed my favorite locust. I have other uses for it, and propose to make it serve me in overthrowing the wave-theory in yet two or three different ways which physicists will hardly fail to appreciate.

As I have just had the pleasure of applying its stridulation to the innocently appearing figures and data of Professor Mayer, and of demonstrating by them that this insect has a physical strength in compressing the air equal to 5,000,000,000 tons mechanical force, I now propose to apply the same music to the figures of Professor Tyndall on the heat hypothesis of Laplace, and will show results in the corporeal energy of this contemptible insect which will throw Professor Mayer and his "$\frac{1}{679}$" additional "density" completely into the shade. I propose to use nothing in this analysis of Professor Tyndall's position except substantial and unquestioned figures and facts, mostly furnished by himself.

The reader, I trust, has not forgotten the emphatic citations from the *Lectures on Sound*, quoted a few pages back, in which this learned physicist explicitly tells us that the "heat" generated by the propagation of a sonorous wave through the air, adds about "one sixth" to the velocity of such sound, and thus accounts for the discrepancy of 174 feet a second discovered by Sir Isaac Newton.

This heat solution of Laplace, it must not be overlooked, is a vital feature of the wave-theory of sound; for, without this formulated augmentation of temperature by the passage of the wave itself in squeezing the air into a "condensation," the theory confessedly falls to the ground, since the observed velocity of sound contradicts it by 174 feet a second, as proved by Newton, and whose calculation all physicists admit to be correct. It therefore becomes essential to the existence of the current hypothesis of sound that the solution invented by Laplace should pass the ordeal of this *stridulation*, or otherwise the bottom falls out of the theory which it professes to rescue from the fatal figures of Newton.

The resort to *heat* by Laplace, in order to add to the elasticity of the air and thus increase the velocity of sonorous propagation, grew out of the observed fact that the general augmentation of the temperature of a mass of atmosphere—as, for instance, by the action of the sun—increases its elasticity, and thus adds to the velocity of sound passing through it. Thus, sound is known to travel about 100 feet a second faster in the heat of summer than in the severest cold of winter, owing solely to the difference in temperature. I will here requote one of the passages referred to, that its teaching may be fresh before the mind of the reader:—

"This *change of temperature produced by the passage of the sound-wave itself*, virtually augments the elasticity of the air, and *makes the velocity of sound about one sixth* greater than it *would be if there were no change of temperature.*"—*Lectures on Sound*, p. 46.

It is impossible to misunderstand the general bearing of this statement, namely, that the effect of a sound in passing through the atmosphere is to squeeze its particles into *condensations*, and thus generate heat enough to add "one sixth" to the velocity of sound, and make up this deficiency of 174 feet a second. Hence, it follows, as the sound of the locust travels with the same velocity as any other sound, it must also generate the same quantity of heat by the compression of the air, or otherwise the tone of this stridulation would fall short of the uniform velocity of sound.

Now, on this universal assumption of physicists and the unquestioned teaching of the wave-theory, that the passage of a sound-wave through the air augments the temperature of the compressed half of such wave sufficient to add 174 feet a second to its velocity, is it possible to arrive at the exact number of *degrees of heat* thus required to produce such augmentation? Is it, then, possible to ascertain the exact amount of *compression* necessary to generate this quantity of heat? And, finally, can we not then arrive determinately at the physical strength of the insect which produces a *pressure* sufficient to generate that amount of heat? I assume that all these conditions are possible, and that Professor Tyndall himself gives us the figures, in the most concise language, by which at least a part of the facts can be determined, while he gives us a sure clue to the remainder. He says:—

"At a temperature of half a degree above the freezing point of water the velocity is 1,089 feet a second; at a temperature of 26.6 degrees it is 1,140 feet a second, or a difference of 51 feet for 26 degrees, that is to say, *an augmentation of velocity of about two feet for every single degree centigrade.*"—*Lectures on Sound*, p. 25.

No one can misunderstand this. Hence, in order to add "one sixth," or 174 feet a second, to the velocity of sound, the locust must necessarily generate sufficient heat to raise the temperature of the *condensed half of its sound-waves* 87 degrees cent., which is half of 174 feet, or *two feet* velocity "for every single degree centigrade."

Here, then, we have no difficulty in gradually approaching the solution of the problem for which we set out, namely, to ascertain from Professor Tyndall the physical strength of this locust, according to the wave-theory, in so compressing four cubic miles of atmosphere, or at least the one half of it, as to raise its temperature 87 degrees, or one degree centigrade for each two feet of velocity thus added.

It only remains now to ascertain what amount of compression or mechanical *squeezing force* must be exerted upon these four cubic miles of atmosphere to raise the temperature of one half of its mass 87 degrees, or enough to add 174 feet a second to the velocity of sound; for, it must not be overlooked that *one half only* of the air is heated above the normal temperature by this squeezing process, while the other half is just as much depressed by the rarefactions. Hence, in estimating the amount of heat the sound of the locust generates, we must be careful to confine our calculation to *one half* of the mass of air permeated by the stridulation, or otherwise we might unintentionally do injustice to this carefully formulated and purely scientific theory!

But I am obliged here to digress a little from the main inquiry, as to the physical strength of the locust according to the facts and figures of Professor Tyndall, though I will soon return with an important collateral fact somewhat elucidated by the digression.

I acknowledge that it will seem a little queer to the unscientific reader how the velocity of sound can be increased by the heat of the "*condensations*," when the "associated *rarefactions*" are just as much *colder* as the condensed portion is *hotter*, since the one would seem naturally to *retard* the sound-pulse as much as the other could *accelerate* it. This, however, is a small-sized problem to the wave-theory compared to some of the difficulties it is obliged to encounter, as the reader no doubt begins to realize. Professor Tyndall appreciates this difficulty, and tries to parry it in his explanation of Laplace's law. He admits if the air were permanently parcelled off into strata alternately hot and cold, in the same manner as it is moulded and divided up by a sound-pulse into condensations and rarefactions, that an extraneous sound passing through these hot and cold layers would receive no augmentation of velocity.

How, then, the common sense of the reader would naturally prompt him to ask, does the law of Laplace make a sound travel any faster on account of this heat and this cold, the one a stand-off to the other, and both equally balanced in the "condensations and rarefactions"? It is not at all clear to the writer how this can take place, even with Professor Tyndall's explanation before him, even supposing such condensations, &c., actually to exist, for a very definite reason, which will soon be given; but the explanation given by the theory amounts to about this: The condensed half of the wave being *hotter* than the normal air increases the elasticity and augments the spring-force of this condensed portion of the atmosphere, which gives greater velocity to the air-particles in their oscillations to and fro; while the rarefaction, being colder, has less elasticity, and thus withdraws resistance or opposing spring-force to the air-particles as they are driven backward from the condensation. In this way the velocity of the particles is increased both by the heat and the cold. The hypothesis of Laplace is surely as accommodating as one could ask.

The whole matter, however, is purely chimerical and absurd, since both Professors Tyndall and Helmholtz have told us that the actual distance the air-particles travel in these oscillations to and fro must necessarily be almost infinitesimally small, possibly not the hundredth or the five hundredth part of an inch. To make these hypothetic oscillations of the air-particles to and fro amount to anything appreciable in the generation of heat and cold, which must be the case in adding 174 feet a second to the velocity of sound, they must necessarily travel more than an infinitesimal distance. And here is where the theory contradicts and annihilates itself utterly, by teaching in the most explicit language that the air-particles do travel a long and measurable distance to and fro, —that the condensations and rarefactions are actually produced by the *travel* of the air-particles—first forward, causing the *compression*, while leaving a partial vacuum which becomes the rarefaction, and then returning, which again produces a condensation in the space just occupied by the rarefaction,—thus alternately converting the same air-particles into condensations and rarefactions *by traveling the entire distance back and forth from rarefaction to condensation, and vice versa.* The language of Professor Tyndall can leave no doubt on this matter:—

"As the pulse advances it *squeezes the particles of air together.*"

"You ought to see mentally the air-particles when *urged outwards* by the explosion of our balloon *crowding closely together;* but immediately behind this *condensation* [Mark it, the "condensation" is caused by the travel of the air-particles in being "urged outwards" and "*crowding closely together,*"] you ought to see the particles *separated more widely apart.* You ought, in short, to be able to seize the conception that a sonorous wave consists of two portions, in one of which the air is *more dense* and in the other of which it is *less dense* than usual."

"Figure clearly to your minds a harp-string vibrating to and fro; it advances, and causes the *particles of air* in front of it to *crowd together, thus producing a condensation of the air.* It retreats, and the air-particles behind it *separate more widely, thus producing a rarefaction of the air.*"—*Lectures on Sound,* pp. 5, 28.—*Heat as a Mode of Motion,* p. 225.

Thus, all the way through the writings of this physicist the *condensation* of the air is caused by the *travel* of the air-particles, while the *rarefaction* is produced by the same travel in leaving a partial vacuum; and, as the same atmospheric space which is now the condensation instantly becomes the rarefaction, and *vice versa,* it follows irresistibly that there is no way of creating alternate rarefactions and condensations in the same mass of air every time a wave passes *except by the same air-particles traveling back and forth the entire distance from rarefaction to condensation,* and *vice versa,* as the two change places.

Let it thus be remembered that the distance the air-particles travel in producing these supposed condensations and rarefactions can not be infinitesimal, if there is any truth in the theory, because their *travel* to and fro creates these condensations and rarefactions, and hence they are obliged to pass the whole distance thus signified, which is simply *half a wave-length,* as is perfectly plain.

Is it not, then, clearly manifest from the foregoing quotations that there can be no condensation of the atmosphere unless the air-particles themselves travel, and thus *crowd* and *squeeze* together as far as the condensation extends, in order to produce it? I have already shown, in various ways, that there is no spring-force in the air by which a pulse can be driven a single inch beyond the actual travel of the air-particles themselves, owing to the exceedingly slow motion of the fork or string and to the extreme mobility of the air, neither of which seems ever to have entered the minds of these savants.

Now, what is the distance, according to the wave-theory, which these air-particles have to travel in order to pass from the rarefaction into the condensation? I have said it must be *half a wave-length,* of course. Professor Tyndall says:—

"The *length of a wave* is measured from the *centre of one condensation to the centre of the next one.*" [See list of quotations, page 79.]

From the middle of a rarefaction, therefore, to the middle of a condensation is half a wave-length. It is thus a simple matter to determine the actual distance the air-particles oscillate "to and fro" in squeezing the air together, and thus forming these "condensations and rarefactions."

The wave-length of a sound depends

on its pitch, or, which is the same thing, on the number of waves per second sent off from the sounding body. If it is a very high sound, like that of the high D of the piccolo flute (4,752 vibrations a second), the length of the wave is less than *three inches*, as can be seen by dividing the number of vibrations as above into the velocity of sound, or 1120 feet a second; whereas, the lowest tone of the organ, as stated by Professor Blacerna in his recent work on sound, has 16 vibrations to the second, and a consequent wave-length of 70 feet! It thus follows that in the sound of such an organ-pipe the air-particles are obliged to travel 35 feet and back 16 times each second, in order to pass from the space occupied by the center of the rarefaction to the center of the condensation and back. They would thus move with a velocity in one direction of 560 feet a second, or at the rate of 381 miles an hour, which would produce a tornado of more than double the velocity necessary to sweep a village into ruins! If there was the least truth in the wave-theory, the sound of a church-organ should get up a cyclone which would blow a cathedral into atoms!

I do not propose to misrepresent these learned physicists in the least in stating the legitimate and preposterous effects of the wave-theory. In fact, it is difficult to misrepresent the theory, say what you will about it, for, in some of its contradictory aspects it will be sure to justify you. I admit frankly that it would seem absolutely to defy belief that any pretended scientific theory should teach in this nineteenth century such a transparent impossibility as that the stridulation of an insect should shake four cubic miles of atmosphere into condensations and rarefactions, and so compress one half of it by squeezing its particles together as to generate this calculated heat of Laplace sufficient to add 174 feet a second to the velocity of sound; and I would not at all blame the reader if he should throw down this volume, charging me with the foulest misrepresentation of these eminent scientists, unless I should continue to demonstrate my assertions beyond the possibility of doubt by quotations from their works couched in such explicit and unmistakable language as to render misconstruction impossible.

I admit the justice and fairness of this course on the part of the reader, and shall therefore continue to fortify every position I take, so that in the end the learned authorities from whom I quote and whose theory I am reviewing shall have no reason to complain. Professor Tyndall says, and I wish the reader to carefully note it:—

"All that you have heard regarding the transmission of a sonorous pulse through the air, is, I trust, still fresh in your minds. As the pulse advances *it squeezes the particles of air together*, and *two results follow from this compression of the air.* Firstly, its *elasticity is augmented* through the mere augmentation of its *density*. Secondly, its elasticity is augmented *by the heat developed by compression.* . . . Over and above, then, the elasticity involved in Newton's calculation, we have an *additional elasticity* due to the *changes of temperature produced by the sound itself.* When both are taken into the account, the calculated and the observed velocity agree perfectly."—*Lectures on Sound*, p. 28.

This is too plain to require comment. But here, remember, as I have already intimated, Professor Tyndall does not teach that the *average temperature* of the atmosphere is changed in the least by this compression or squeezing of the air-particles together. He carefully guards against such a result as too superficially absurd to be taught even by the wave-theory. He has provided against this in a score of places by reiterating, as already quoted so often, that each condensation of a sound-wave is accompanied by a counterbalance in the shape of an "associated rarefaction," and hence that in the latter the temperature is as much depressed as it is raised in the former, thus keeping the average temperature the same. He remarks:—

"The average temperature of the air is unchanged by the waves of sound. We can not have a condensed pulse without having a rarefied one associated with it. But in the rarefaction the temperature is as much lowered as it is raised in the condensation."—*Lectures on Sound*, p. 29.

This really seemed to be quite a necessary precaution on the part of the wave-theory, or otherwise it would be impossible for a katydid to stridulate without making the surrounding atmosphere so nearly incandescent that nobody could live in it! Hence, the necessity of rarefactions as *cold* as the condensations are *hot*.

But what does this precaution amount to, after all? We here have it distinctly taught that every particle of the air through which a sound passes is first *heated* to this very temperature requisite to add 174 feet a second to the velocity of sound before it can be cooled by the associated or succeeding "rarefaction"! And I have just shown, from Professor Tyndall, that, in heating a given mass of the atmosphere ordinarily, as by the effects of the sun, the same as if the whole mass were a condensation, it must actually be raised 87 degrees centigrade (156.6 degrees Fahrenheit) to add the 174 feet a second, or at the rate of one degree to each two feet of additional velocity! Thus, one half of the entire atmosphere throughout the four cubic miles is heated all the time and the other half cooled all the time while the locust is stridulating, though there is a transition and a transference of the heat from one to the other half constantly taking place, according to the wave-theory. Yet this assuredly can not make the amount of heat and compression less than one half what it would be if both halves of the atmosphere were heated at once.

But here I meet with a difficulty in my calculation, and the only one I have yet encountered. Professor Tyndall does not tell us what amount of "*pressure*" to the square inch is necessary to generate a definite amount of heat, or to raise the mercury in a centigrade thermometer, say, *one degree*. This was a great neglect, and an almost unpardonable oversight, under the circumstances. He explicitly tells us how many degrees of heat it takes to add a given number of feet per second to the velocity of sound when the whole atmosphere is heated, as I have already quoted, namely, 87 degrees centigrade for 174 feet, or one degree for each two feet of velocity. He is also very careful to tell us that the "condensation" of a sound-wave really does generate the requisite heat, by *squeezing the air-particles together*, to add these 174 feet a second. But he there stops, leaving us entirely in the dark as to how much this *pressure* actually amounts to in pounds and ounces! Had he told us this, we should be able to know all about the strength of the locust in one minute.

During his lectures on *Heat as a Mode of Motion* (page 82, first edition), he shows how much *weight* an inch-column of air will support while being heated up to any number of degrees, and thus kept at constant volume, without any change in its density. But this is a very different thing from the generation of heat by squeezing the air-particles together and thus augmenting its *density* as well as its *elasticity*, the same as sound-waves are claimed to operate.

He even goes so far as to show his audience how to generate this heat by the *compression of the air* in a glass tube, and actually does generate heat enough to ignite a piece of amadou by a quick and powerful motion of the piston! Still, he remains stoically taciturn upon this paramount question as to the *amount* of pressure to the square inch, in avoirdupois, which would be required to raise the mercury, for example, a single degree.

This is the very thing, above all others, he should have attended to in his lecture, and thus have enabled his hearers and afterward his readers to form some sort of an estimate of the mechanical force exerted to send off a given system of sound-waves, thus to produce their condensations, and thus to generate the required heat for the 174 feet a second additional velocity, according to the formula of Laplace.

Professor Mayer was not afraid! He pluckily came right out and told us in the plainest vulgar fractions that a given sound in passing through the atmosphere and producing its condensations actually increased the "density" of the "compressed half" of the wave "$\frac{1}{679}$" over the normal density of the air, which left it a simple mathematical problem to calculate the physical strength of the locust in thus increasing the "density" of the one half of four cubic miles, which we have readily found to be 5,000,000,000 tons! But it really looks as if Professor Tyndall was afraid. If he had known how much mental anxiety he would have saved the writer by giving this small piece of information, he would surely not have been so selfishly inconsiderate as to withhold it.

Seriously, why was it that Professor Tyndall so signally neglected to give this important basis of calculation while discussing the very question where it would so appropriately have come in? Either he did not know himself how much pressure to the square inch of air was necessary to generate one degree of heat, or else he knew and did not care to tell his audience and readers! To suppose that he knew, but intentionally suppressed this important piece of information, at this critical juncture of his course of lectures, when he could so easily have imparted the valuable intelligence in the compass of a single short sentence, would be extremely ungenerous. I shall therefore assume that *he did not know*, and had not even an approximate idea as to the *physical pressure* it takes in pounds and ounces to raise an inch-column of air one degree centigrade, even when the air is confined within a tube so that it can not exercise its *mobility* and get out of the way, *to say nothing of the inconceivable difficulty of producing such compression in the free air!* I adopt this charitable view, on the supposition that had he been aware of this mathematical fact he might have spoiled a splendid lecture by suddenly discovering, on imparting the information to his audience, the utter baselessness and absurdity of the whole wave-theory, and unceremoniously have left the platform in mortification and disgust. I am sorry, in one sense, that the thing did not occur; for, had the idea flashed across his mind at that stage of the investigation, being but the first lecture of his course, and had the actual physical truth of the matter impressed itself upon him, as it will soon be impressed on the reader, I have faith enough in the intrinsic candor of the man to believe he would have at once abandoned the wave-theory as a monstrous scientific fallacy; and, in all probability, the writer of this review would have been spared the unpleasant task of holding up to the light the escapades and fiascos of his fellow-workers in science, by having his labors anticipated in a much more elegant and accomplished manner.

I may add here, in extenuation of the manifest lack of knowledge on the part of this eminent lecturer, that I have sought in vain among my scientific friends for the same information as to the amount of pressure to the square inch of atmosphere which would be necessary to raise the temperature one degree, while I was equally unsuccessful in consulting authorities, after examining all the works on pneumatics within my reach. I was at last compelled, as a *dernier ressort*, to construct an instrument especially adapted to the purpose of testing this important scientific question,—important both to me in the present discussion, and to the future status of the wave-theory, as well as to the cause of science generally. I will briefly describe the instrument, which is exceedingly simple, and then give the result of the experiment.

It consists of a glass tube of any convenient length, so it is long enough to admit a small thermometer at the bottom, and of a diameter equal to one square-inch cross-section, into which a piston is accurately fitted so as to work air-tight, by means of which the atmosphere may be compressed to any required extent. In making the test I had only to drop the thermometer into the tube, which, being wholly inclosed within the compressed air would sensitively respond to the generated heat for any given movement of the piston.

The result was that on suddenly pushing down the piston a distance equal to one half the depth of the tube (thus giving the other half of the column two atmospheres, or a pressure around the thermometer of about 15 pounds to the square inch), the mercury indicated an elevation of about two and a half degrees centigrade; but as the radiation of the heat through the surrounding tube would be probably equal to its action on the glass of the thermometer, I called the heat actually generated five degrees by a pressure of 15 pounds to the square inch, in order to do ample justice to the wave-theory.

We thus experimentally and mathematically supply the deficiency caused by the inexcusable neglect of Professor Tyndall, and arrive, at least, at the approximate pressure in pounds necessary to raise the temperature of the condensed half of a supposed air-wave 87 degrees centigrade, which we are assured by Professor Tyndall is the augmentation required to add 174 feet a second to the velocity of sound. Of course, this is on the basis that each supposed air-wave is inclosed within a tube and acted on by a piston.

The question may be simply stated as follows: If a cubic inch of air requires 15 pounds pressure (reducing it to one half its bulk) to raise its temperature 5 degrees, how much pressure will it require to raise the temperature of the same cubic inch of air 87 degrees? The result can be obtained thus: $87 \div 5 = 17$ (rejecting fractions) $\times 15 = 255$ pounds. Thus, if there is any truth in the wave-theory, we have in plain figures arrived at the astounding fact that a sound of any kind in passing through the air must produce an atmospheric pressure in the condensed portion of its waves of 255 pounds to each cubic inch in order to raise its temperature 87 degrees centigrade, which, as we learn, is necessary to add 174 feet a second to the velocity of sound, and thus save the wave-hypothesis from utter destruction at the hands of Sir Isaac Newton!

In this simple experiment we have completely remedied the defect of Professor Tyndall's lecture by getting at the approximate if not actual pressure produced on the condensed half of the sound-wave in order to generate this required heat of Laplace, the very point above all others he should have been particular about explaining, so that the unscientific reader might be able to ascertain exactly how many *tons pressure* upon the atmosphere of his sleeping apartment a *mosquito*, for example, exerts by serenading him with its hateful music! The Professor ought to thank the writer for correcting this important defect in his book, and for thus having furnished him the proper scientific data for his next course of lectures on sound. For, as all sounds travel 174 feet a second faster than they would if there were no heat generated by the *condensations*, or if there were no *squeezing of the air-particles together* by the passage of the sound-wave, it follows that the mosquito's sound is likewise augmented in velocity in the same way and to the same degree; and, as we have just found that it takes 255 pounds pressure on a cubic inch of air to raise its temperature 87 degrees (the required heat for 174 feet additional velocity), any reader can easily make the necessary calculation as to the absolute mechanical pressure which a mosquito must produce throughout a room of given dimensions in order to generate sufficient heat to thus add "one sixth" to the velocity of its sound.

Let us see. As our experiment demonstrates 255 pounds pressure to the cubic inch as the mechanical force necessary to generate the required 87 degrees of heat, it follows, as a mosquito can be heard in a still night throughout a room ten feet square, it must therefore exert this amount of pressure on *one half* of all the cubic inches of air in the room, since one half is compressed while the other half is rarefied. The room contains 1,728,000 cubic inches, the compressed half of which (864,000) multipled by 255 pounds pressure makes the mechanical energy of this insect 220,000,000 pounds, or a physical force exerted on the atmosphere of the room by the motion of its wings of *one hundred and ten thousand tons!* No advocate of the wave-theory can successfully contradict this result.

The reader need not take these figures on my authority, but can make the calculation for himself, taking only the undisputed data furnished by the authoritative physicists from whom I have quoted, in connection with the amount of pressure necessary to raise the temperature of air 87 degrees, as determined by scientific experiment. He will thus form an accurate and comprehensive idea of the physical strength of this *dipterous proboscidian*, according to this highly philosophical theory which has stood "unshaken" for hundreds if not thousands of years!

Applying the same data to the sound of the *locust*, which permeates *four cubic miles* of air instead of that contained in an ordinary bedroom, the reader at once sees the almost infinitely ridiculous and tantalizing character of the result. Yet, as preposterous as it is, it is no more so than the wave-theory, which furnishes the undeniable basis for the calculation. Professor Mayer's estimate, based on the important discovery which he announces, namely, that sound compresses one half of the wave enough to add "$\frac{1}{879}$" to the normal "density" of the atmosphere, only puts the physical strength of this insect at the modest amount of *five thousand million tons;* whereas the calculation of Professor Tyndall, based on the estimated heat which this pressure must necessarily generate to meet the requirements of Laplace, throws these figures utterly into the shade, making the physical energy of the locust equal to 132,566,207,938,560,000 pounds, or, in round numbers, 66,000,000,000,000 tons, being exactly *thirteen thousand two hundred and fifty-six* times greater in mechanical effect than the estimate of his American collaborator! These learned physicists can settle the matter between them.

But here I imagine the reader saying: "Although you have shown from the highest authorities that the compressed half of the atmosphere through which a sound passes is really *raised in temperature*, according to the wave-hypothesis, *by the squeezing of the air-particles together;* and although you have proved beyond question that this theory teaches as one of its fundamental principles that the heat thus generated is necessary to make up the discrepancy of 174 feet a second in the calculated velocity of sound, as discovered by Sir Isaac Newton; and notwithstanding you have shown from Professor Tyndall that where the atmosphere is warmed in a mass, as by the action of the sun, it requires one degree centigrade for every two feet velocity added, or 87 degrees for this deficit of 174 feet;—still, are you not mistaken about applying the same ratio of augmented heat to the compressed half of the sound-wave? Is it not possible that a much less elevation of temperature than 87 degrees would suffice for heating these condensations, and making good this deficiency, according to the formula of Laplace and the solution of the problem as expounded by Professor Tyndall?"

I am willing, for the sake of the argument, to concede the possible correctness of this objection, and to agree that Professor Tyndall does not say that the same degree of augmentation is requisite in both cases. Yet reason certainly tells us that if there is any difference at all, the compressed half of the sound-wave should require the greater augmentation of heat to affect this 174 feet velocity, since it is always found in close juxtaposition with a chilled "rarefaction," which Professor Tyndall assures us is just as much *colder* than the normal atmosphere as the "condensation" is *hotter!*

The bare fact that this learned scientist, in all this discussion of Laplace's solution, occupying some eight or ten pages of his book, does not say a single word as to how many degrees of heat these condensations generate which adds 174 feet a second to the velocity of sound, in connection with the important consideration that he distinctly teaches in other places that the air, if heated by the sun, would require 87 degrees centigrade to make up this deficit of 174 feet a second, is a sufficient proof to every fair-minded man that he intended the reader to understand—if he knew himself, and if he intended to convey any definite idea on the subject—that the amounts of heat requisite for a given augmentation of velocity would be the same in both cases, or otherwise he would have pointed out the difference between them. Will not the intelligent judgment of every unbiassed physicist acquiesce in this as the only logical conclusion? On the supposition that Professor Tyndall really possessed the knowledge, the fact of his silence on this vital question as to the exact amount of heat generated in the compressed half of a sound-wave can be only accounted for on the ground that he wished and expected us to understand that the "condensation" required the same augmentation of heat by pressure to add 174 feet a second that the entire atmosphere would require if heated by the sun, as he had so fully explained in other places.

But I am willing to be accommodating to any reasonable extent, since I feel entirely able to make any concessions which a candid physicist would be willing to ask, and still annihilate this preposterous formula of Laplace, so conspicuously put forward and advocated by Professor Tyndall as lying at the foundation of the wave-theory, since without these "condensations and their associated rarefactions," with their resultant heat and cold, he frequently gives us to understand that sound-waves could not exist.

I am therefore ready to suppose that instead of the compressed portion of the sound-wave being raised in temperature 87 degrees with a squeezing force of 255 pounds to the cubic inch in order to add this required 174 feet velocity, it is only necessary that it should be raised *one* degree! I wonder if Professor Tyndall and my doubting reader would be satisfied with this reduction? If not, no philosopher shall excel me in scientific liberality, and I will therefore concede, to oblige this hypothesis, that the augmentation of heat in the compressed half of the wave, which adds "one sixth" to the velocity of sound, instead of being 87 degrees, as it ought to be, is *but the one millionth part of one degree!* Will this be sufficient? If Professor Tyndall were present and should require it, I would gladly reduce it still further, for I am certain that any possible reduction he would be willing to ask, as a physicist, would still make the solution altogether too *hot* for the wave-theory!

On this new basis, then, that the *one millionth part of one degree* is all the heat there is contemplated in this famed solution of Laplace, and all the heat there is generated in these boasted "condensations" of the wave-theory of sound, and that this almost inconceivably minute augmentation was all Professor Tyndall had in his mind as being sufficient to add "one sixth," or 174 feet a second, to the velocity of sound (which is entirely insupposable on its very face), and we still find, by incontrovertible figures, that the locust exerts on the atmosphere permeated by its sound a mechanical pressure of *seven hundred and fifty-eight thousand tons*, or a physical force equal to that of all the locomotives in the United States! Are physicists ready to accept this absolute showing of the wave-theory after thus modifying the true calculation which the hypothesis warrants, by *eighty-seven million subdivisions?*

All these calculations, as before intimated, are based on the mechanical experiment of generating heat by compressing the air in a tube when so confined that its mobility can not come into play. If I should assert that the same movement of the piston which generates five degrees of heat in the atmosphere of the tube, would not, if made in the open air, produce the thousandth part of one degree in augmentation, or one 5,000th part as much, owing to the mobility of the atmosphere and its freedom to get out of the way and thus escape compression, I would only assert what the intuition of every physicist would indorse as undeniably true. If this is a correct representation of the matter, then it follows that the foregoing calculations present less than *the one five thousandth part of the actual absurdity of the wave-theory!*

These are not misrepresentations, nor are they even exaggerations of this unfortunate hypothesis. Taking any of the assumed facts put forward and relied on by physicists as fundamental to this theory, and it is almost impossible, using them as a basis of calculation, to draw any deductions or employ any figures which will exaggerate the incongruity of the hypothesis. It is therefore extremely difficult to do the theory injustice, say what you will about it, for, when looked at in the light of reason and with the slightest respect for the laws of mechanics or the relation subsisting between mathematics and philosophy, the supposition that an insignificant insect, *by moving its legs in the free air,* can actually produce such an atmospheric compression as to generate any appreciable heat at all, even an inch around it, to say nothing of so augmenting the temperature throughout four cubic miles as to add 174 feet a second to the velocity of sound, becomes too infinitely ridiculous and insane a supposition to admit of being discussed with any degree of patience. Yet, under the circumstances, I have tried to keep cool even while battling with such a scientific monstrosity, since the theory has to be discussed and its foundationless character pointed out, owing to the fact that it is advocated as science by every physicist who has written on the subject, taught as science in our schools and colleges, and is honestly believed in as science by the ablest and most scrutinizing intellects of the world. Still, with all my efforts to the contrary, when seriously controverting such Mother-Goose nonsense under the disguise of natural philosophy, I can not help feeling at times an indefinable sensation of disgust mingled with astonishment. I shall nevertheless continue on in the work of fighting as one that beateth the air, perhaps as much to the disgust of modern physicists as to myself. For the reader must be aware, unless I have been guilty of the most deliberate and barefaced falsification of the eminent authorities from whom I have quoted (a question admitting of easy verification or disproof), that there is no possible way for them to escape the merited condemnation and even ridicule of future scientists except by publicly acknowledging themselves mistaken, and thus summarily renouncing one of the most transparent fallacies ever taught as science.

Conclusive, however, as have been the foregoing arguments, they will be more than paralleled in effectiveness by those soon to follow,—showing that in numberless ways, and viewed from every conceivable standpoint, the same uniform impossibilities come to the surface. It is not possible, in fact, to look at this fundamental idea of the wave-theory, namely, that a sound-pulse is constituted of an atmospheric "condensation and rarefaction," —an assumption, by the way, on which the entire hypothesis hinges,—without seeing "absurdity" written all over it.

As one illustration of what I have just said, I would name the fact that Professor Tyndall distinctly though unwittingly teaches, as the necessary result of such a "condensation and rarefaction," that *two unison sounds must travel together with considerably greater velocity than either one of them would travel alone!* He teaches this, as I will now demonstrate, because the very idea of a sound-wave, constituted of a *condensation* and *rarefaction* of the air, involves it; and as both Professors Tyndall and Helmholtz tell us that the only sound-wave possible to exist consists in this *condensation* and *rarefaction* of the atmosphere, as already quoted (see page 125), it follows that the above palpable contradiction of the observed velocity of sound turns out to be a feature essential to the existence of the wave-theory. Let us now examine the evidence on which my position is based.

In the first place, Professor Tyndall tells us that two unison sounds traveling together, with their waves *coinciding*, must positively *quadruple* their *loudness* by quadrupling their *condensations* and *rarefactions;* and by thus making these characteristics *fourfold*, they quadruple the amount of *heat* generated in the compressed portion of the wave as well as quadruple the amount of cold developed in the rarefied portion. And as I have already shown, from both Professor Tyndall and Laplace, that an ordinary sound generates, by condensing the air, *heat* enough to add 174 feet a second to its velocity, then, evidently, if two sounds together produce *four times the loudness* and *four times the condensation* or *compression* of the air, it must generate four times the amount of *heat* and *cold*, and consequently must add *four times* this augmentation of velocity, or, in other words, must add four times 174 feet per second! Is not this unavoidable?—that is, if Professor Tyndall teaches, as I have asserted, that two unison sounds produce four times the *condensation* of the air that one does?

I now invite the reader to the proof, which is too plain to be misunderstood:—

"It is easy to see that the forks may so vibrate that the *condensations* of the one shall *coincide* with the *condensations* of the other, and the *rarefactions* of the one with the *rarefactions* of the other. If this is the case, the two forks will assist each other. The *condensations* will in fact become *more condensed* and the *rarefactions* more *rarefied*, and as it is upon the difference of *density* between the condensation and rarefaction that *loudness depends*, the two forks, thus supporting each other, will produce a sound of greater intensity than that of either of them vibrating alone."—*Lectures on Sound*, p. 258.

This, as far as it goes, is exceedingly concise and to the point. What it lacks in positive proof will soon be supplied. Mark, however, the teaching of this citation. Two unison sounds traveling together, so that *condensations* coincide with *condensations* and *rarefactions* coincide with *rarefactions*, not only make the condensations "more condensed" and the rarefactions "more rarefied," but the "loudness" is thereby increased in the same proportion, since "*it is upon the difference of density . . . that loudness depends.*" But how much is this "loudness" and "density" increased by two systems of waves thus coinciding? Professor Tyndall shall answer:—

"If in two systems of sonorous waves *condensation coincides* with *condensation* and *rarefaction* with *rarefaction*, the sound produced by such coincidence is *louder* than that produced by either system taken singly."—"If the two sounds be of the *same intensity, their coincidence produces a sound of four times the intensity of either.*"—*Lectures on Sound*, pp. 284, 285.

Hence, we have here the conclusive proof of my position, namely, that two sounds traveling together, with their waves coinciding, must necessarily produce *fourfold* the *condensation* of either traveling alone, since the Professor distinctly tells us that the *loudness* or *intensity* of the sound is quadrupled, while at the same time assuring us that it is upon the *difference of density* that *loudness* depends. Now, as the heat generated by these condensations is exactly in proportion to the "density" or *compression* of the air, as all physicists agree, and since the augmentation of velocity, according to Laplace, by which 174 feet a second is added to the speed of sound, is caused by the *heat* generated in these condensations, it follows irresistibly that since the *loudness*, the *density*, and the *heat* must all be quadrupled, this augmentation of velocity (174 feet a second) must also be quadrupled, making this added velocity on account of two sounds traveling together 4 times 174, or 696 feet, which, added to Newton's calculated velocity (916 feet), actually makes the velocity of the two sounds united 1612 feet a second at the freezing temperature, instead of 1090 feet, as all observation proves! These are figures which will neither *lie* nor *contradict* themselves, wha ever the wave-theory may be in the habit of doing.

Thus, it unanswerably follows, if these *condensations* and *rarefactions*, being the very foundation of the wave-theory, really exist at all, that two sounds coinciding must necessarily travel together 522 feet a second faster than either sound can travel singly! But since all observation shows that there is not the slightest difference in the velocity of sound, whether a single tone or a dozen tones pass through the air at one time, it demonstrates that no such thing as *condensations* and *rarefactions* occurs in the propagation of sound, thus shattering in another way the very foundation of the theory. Is it possible that any inductive course of reasoning can be more logically clear and demonstrative?

It would really seem that a physicist of such reputed caution in his investigations of science as Professor Tyndall, and who has so often helped other people out of scientific pitfalls and quagmires, would have been able to detect the monstrous character of the fallacy into which he has here inadvertently slipped. One would have thought that so shrewd a scientific thinker, when formulating this proportionate relationship between the "density" of the air, the loudness of tone, the generation of heat by these condensations, and the augmentation of velocity by this heat, all directly connected together and dependent the one upon the other, would have seen their suicidal effect just pointed out, by the very mental effort required to put the erroneous proposition into form. The very fact that he did not detect the self-annihilating character of the hypothesis while writing it out, preparatory to his lecture, only goes to illustrate the blinding effect of a false theory even on the greatest of intellects.

But we have not yet reached the culmination of this error, nor have we even begun to unfold its astonishing results. Even Professor Tyndall can hardly help being amused at the laughable predicament in which his logic has involved the wave-theory. Let the reader carefully follow me for a little, and see some of the beautiful scientific consequences of this hypothesis which has stood unshaken for so many centuries.

As it is upon the *difference of "density"* that "*loudness depends*," (see last quotations,) it follows that just in proportion as the loudness of a tone increases, exactly in that proportion will the air-waves be condensed, exactly in that proportion will the heat be augmented, and exactly in that proportion will the velocity of the sound be augmented. No one can doubt this as being the unavoidable teaching of the theory when its different members are articulated.

Take, for example, a tuning-fork, as possessing a remarkable diversity in range of intensity,—from almost inaudibility, as when held in the hand, to a tone at least of *a hundred times the loudness* when placed on its resonant case, as any acoustician will admit, since it can be heard at a hundred times the distance.

Now, as the fundamental law of the theory assures us that the faintest tone of this fork, as when held in the hand, must necessarily generate sufficient heat by compressing the air to add the required 174 feet a second, or otherwise the velocity of its sound would not conform to observation, it follows that its full tone on its resonant case, if a hundredfold in loudness, must generate one hundred times as much heat by producing one hundred times as much "*compression*" or "*density*" of the air, which unavoidably leads to the conclusion that such a tone must receive one hundred times this additional augmentation of velocity, or, in other words, must have added 100 times 174 feet a second to its normal velocity of 916 feet, as calculated by Newton when no generated heat is included in the estimate, making such aggregate velocity 18,316 feet per second! Any tyro in mathematics can verify this computation by merely passing these figures in review.

What, now, can physicists say in reply

to this *reductio ad absurdum?* If they admit that one hundred times the *loudness* is caused by one hundred times the "*density*" or compression of the air, as they are forced to do, since "it is upon the difference of *density* that *loudness* depends," then, as the amount of heat generated also depends on the amount of this density or compression of the air, the same as the amount of added velocity depends upon the amount of heat generated, there seems to be no possible escape from the foregoing general conclusion, namely, that the velocity of sound must increase exactly in the ratio of its loudness, which flatly contradicts observation! A startling illustration of this fallacy will be adduced at the close of the next chapter, furnishing a demonstrative overthrow of the wave-hypothesis, which no man can gainsay.

But even this logical example of *reductio ad absurdum* is but a small fraction of the trouble in which these physicists have involved themselves and their theory by attempting to build upon this fundamental error of "condensations and rarefactions," and in assuming to utilize their hypothetic heat and cold to get rid of Newton's tantalizing discrepancy. I have another legitimate and irresistible deduction to make from this foundation-law of the theory which must settle even Professor Tyndall, unless the figures already adduced on the stridulation of the locust have paralyzed his mathematical and mechanical susceptibilities.

The reader must not for a moment lose sight of the fact, during the progress of the argument, that this physicist distinctly tells us, and repeats it in many forms, that it is upon the difference of "*density*," or the *compression* of the air by a sound-wave, "that *loudness depends*," and that it must be also upon this same difference in "density" that the generation of heat and the consequent augmentation of velocity depend. If the augmentation of velocity is caused, as the theory teaches, by the augmentation of heat generated by the condensation of the sound-wave, on which *loudness* depends, does it not necessarily follow that the augmentation of velocity and the loudness of sound must keep up a corresponding ratio of increase or decrease? This must be so, or else there is not the least foundation for the formula of Laplace, and no truth in the hypothetic *condensations* of the air and their resultant *heat*, as assumed by Professor Tyndall. But if the augmentation of *velocity* corresponds to the augmentation of *heat*, as Laplace and Tyndall assume, and if the augmentation of *heat* corresponds to the increase of *density*, on which *loudness* also depends, then evidently the various augmentations form a logical chain from one to the other which can not be broken without severing the wave-theory from its base. This relationship being unavoidable, if there is any truth in the assumption of "condensations and rarefactions" and their resultant heat and cold, it is impossible to ignore the conclusion that the velocity of every sound must exactly correspond with its intensity, or, in other words, must increase or decrease with its loudness. Hence, we are brought to the most astounding development of the wave-theory, namely, that since the loudness of sound decreases as the square of the distance from its source, as Professor Tyndall assures us, its velocity must also decrease in like proportion!

I now propose to let this high authority on sound state this ratio of decrease in loudness in his own way, which must necessarily give the corresponding decrease in the condensation produced by the sound-wave, in the heat produced by the condensation, and in the augmentation of velocity produced by the heat, after which it will take but a few moments to point out the fatal effect of his figures. I quote, as usual, from his *Lectures on Sound:*—

"You have, I doubt not, a clear mental picture of the propagation of the sound from our exploding balloon through the surrounding air. The *wave of sound expands on all sides*, the motion produced by the explosion being thus diffused over a continually augmenting mass of air. It is perfectly manifest that this can not occur without an *enfeeblement of the motion*. Take the case of a shell of air of a certain thickness with a *radius of one foot*, reckoned from the centre of explosion. A shell of air of the same thickness, but of *two feet radius*, will contain *four times the quantity of matter;* if its radius be *three feet* it will contain *nine times the quantity of matter;* if *four feet* it will contain *sixteen times the quantity of matter*, and so on. Thus the quantity of matter set in motion augments *as the square of the distance* from the centre of the explosion. The *intensity* or *loudness* of sound *diminishes in the same proportion*."—*Lectures on Sound*, p. 10.

The above can not be misunderstood. The loudness of any tone four feet from the sounding body, according to this law, is but *one sixteenth* as great as directly at the sounding body. Hence, the "density" or "condensation" of the air, and the generation of heat, as well as the resultant augmentation of velocity, are all reduced in the same ratio. This is perfectly manifest, since the augmentation of velocity depends upon the amount of generated heat, the heat depends upon the amount of compression or "density," while "it is upon the difference of *density* that loudness depends." Now, all we have to do is to estimate the decrease in loudness by this same ratio, "as the square of the distance" from the sounding body to the limit of audibility in case of any sound, and we can determine the exact difference in its "condensation" of the air at its start and at its termination, since the decrease in "density" corresponds exactly to the decrease in "loudness;"—we can also determine the exact difference in the amount of heat it generates at its start and also at its extreme limit of audibility, because the ratio of decrease in heat depends upon the ratio of decrease in compression;—and finally, we can also determine the exact difference between the velocity of any sound at its start and at its point of final inaudibility, because the decrease in augmented velocity depends on the decrease in augmented heat, exactly the same as heat depends on the *compression* of the air-wave, or as loudness depends on this "density"!

These premises and conclusions are as immovable (assuming the truth of the wave-theory) as the principles and laws demonstrated by the Copernican System of Astronomy; and, on the supposition that the wave-hypothesis is true, the above chain of ratios must hold good in all its details. Let us now apply this self-evident logic of the theory to the well-known velocity of sound, and see its annihilating result.

According to this law laid down by Professor Tyndall, a sound, after passing a distance of 100 feet from the sounding body, would have but one 10,000th the intensity or loudness as at its source, since you have simply to multiply 100 or any other number by itself, the same as Professor Tyndall multiplied 4 by itself in order to determine this ratio of decrease for any distance. It follows, therefore, if Professor Tyndall is right, that the steam siren (employed along the coast in our signal service), which can be easily heard at sea a distance of ten miles, or 52,800 feet, when the conditions of the atmosphere are favorable, would actually possess, in round numbers, but the one 2,000,000,000th as much intensity or *loudness* at a distance of ten miles as at the start! Using Professor Tyndall's measure of "*feet*," as he does, in ascertaining the ratio of this sound's decrease (which we must do, of course, when such high authority prescribes it), we have only to multiply the 52,800 feet into themselves to determine this proportion of decrease in intensity, as the square of the distance from the source of the sound, thus obtaining the infinitely incredible if not preposterous result demonstrated above. But, for the present, let us accept these figures as correct, since they legitimately belong to the wave-theory, and see what they will do for the hypothesis.

Since the sound of the steam siren at a distance of ten miles must necessarily have, according to the above ratio, but the one 2,000,000,000th as much "*loudness*," it can accordingly generate but the one 2,000,000,000th as much *heat*, since the *heat* and the *loudness* alike depend on the "density" or the *compression* of the air, and must therefore exactly correspond to it in these respects and to each other. And, finally, the sound at that distance would receive but the one 2,000,000,000th as much augmentation of *velocity*, according to Laplace, on account of this reduced augmentation of *heat*, as at its source, where it is, of course, 2,000,000,000 times as loud, causing 2,000,000,000 times as much density or compression of the air, and consequently generating 2,000,000,000 times as much heat! Are physicists prepared for this?

Possibly, if I should invert this statement of the problem, beginning ten miles away from the steam siren, and then trace the sound backward toward its source by applying the same law to find the *increase* by which Professor Tyndall determines the *decrease*, since they are evidently the same in ratio "as the square of the distance," it might be possible to make the infinite audacity and nonsense of the wave-theory more intelligible to these astute physicists whom I have the honor of reviewing. Let us look at it in this light for a moment, and note the consequences.

At the extreme limit of the ten miles we will suppose, as we are of course obliged to do to accommodate this hypothesis, that the sound of the siren, being still distinctly heard, must necessarily produce *sufficient condensation* of the air to generate *sufficient heat* to add this required 174 feet a second to its velocity, or otherwise the sound would not travel according to observation; and, what is worse than that, it would contradict Professor Tyndall and overthrow the formula of Laplace which accounts for "one sixth" of the velocity of sound, or 174 feet a second, by this generation of heat.

If, then, the sound, ten miles away from the siren, still generates *heat* enough to add this 174 feet a second to its velocity, which it must do if there is any truth in the wave-theory, it follows, as a self-evident proposition, since the sound increases in loudness as we trace it backward toward its source by Professor Tyndall's law, "as the square of the distance," that it increases in its augmentation of *heat* and *velocity* in the same proportion!

There is no escape from this, for we can almost use the Professor's own words, and say: At 2 feet from this ten-mile limit, passing toward the siren, the sound is 4 times as loud; at 3 feet it is 9 times as loud; at 4 feet it is 16 times as loud; at 10 feet it is 100 times as loud; at 100 feet it is 10,000 times as loud; and at 1,000 feet it is, of course, 1,000,000 times as loud! Yet 1,000 feet nearer the siren, at such a remote station (less than the *fiftieth* of the distance) would evidently not make a difference in the loudness of the sound which could hardly be detected by the most sensitive ear, though Professor Tyndall's highly scientific (!) formula makes the sound increase to *one million times the intensity* in this comparatively trifling space! Can a theory be worthy of this enlightened age, or make any claim upon the intelligence of the reader as a scientific hypothesis, which depends for its existence on the inculcation of such a monstrous fallacy of science as this ratio of decrease in sound, gravely formulated by this eminent physicist?

But continuing to trace the increasing sound backward toward the siren, we not only have it 1,000,000 times as loud, according to this brilliantly formulated ratio, when we have gone only 1,000 feet nearer to the source of the sound, but, as shown when pointing out the proportion of decrease as we receded from the siren, the sound unavoidably becomes 2,000,000,000 times as loud directly at the instrument as it is ten miles away. Then it necessarily follows that it must produce 2,000,000,000 times as much *compression* or "*density*" of the air at the instrument (since "it is upon the difference of *density* that *loudness* depends,") as it does ten miles away,—that it must generate 2,000,000,000 times as much *heat* at the instrument as it does ten miles away; and, finally, that the augmentation of velocity caused by such generated heat, according to the hypothesis of Laplace, must be 2,000,000,000 times as great at the instrument, or, in other words, it must produce an augmentation of 2,000,000,000 times 174 feet a second, which, independent of the normal velocity without heat (916 feet), absolutely makes the velocity of sound as it leaves the mouth of the steam siren, 348,000,000,000 feet, or 66,000,000 miles a second, *being more than three hundred and forty-seven times the velocity of light!* Are physicists prepared for this? Whether they are or not, it is the unexaggerated teaching of the wave-theory, to which Professor Tyndall is irrevocably committed by his ratio of the increase or decrease of loudness as the square of the distance from the sounding body.

No man who accepts the current hypothesis of sound as expounded by Professors Tyndall, Helmholtz, and Mayer, and in fact all who have written on the subject, can call in question the legitimacy or logical necessity of the results just arrived at, or deny but that they are the unavoidable outgrowths of the wave-theory. However fabulous the foregoing array of figures may seem, we are nevertheless obliged to accept it as representing the well-authenticated facts of philosophy and science so long as the current hypothesis of sound is looked upon and permitted to exist as a scientific theory. Shall it continue to be so looked upon and be so permitted to exist? is the important question here submitted for the decision of the scientific world.

At this juncture of the discussion an opportunity offers, which, perhaps, may not so readily occur again, for a brief exposition of the new hypothesis of Substantial Sonorous Pulses, in order to show how beautifully and consistently it solves this problem of the decrease of intensity in Sound, Light, and Heat, as the true square of the distance from their source.

This conception that sound consists of substantial corpuscles instead of being constituted of the undulatory motions of the *medium* through which it passes, was fully elucidated in the discussion of sonorous reflection and the falling pitch of a passing locomotive-whistle at pages 117, 122, 123, 124.

According to the views there presented, it is but a simple matter to mentally view the particles of sonorous substance radiating from a sounding body in all directions, becoming less and less in number, or, in other words, becoming sparcer and sparcer the farther they advance, as the square of the distance from the center. Neither is there any necessity for supposing that such sound-atoms cease in their travel or retard in their velocity in the slightest degree when they cease to be audible, or, in fact, until they reach the extreme limits of the medium which conducts them. But as it requires a certain quantity or number of these particles to come into contact with the tympanic membrane in order to affect audition, it rationally follows that the *range* of a sound, or the distance at which it can be heard from its source, depends upon the density or number of these particles generated and set free by the sonorific body, or, in other words, depends on the compactness or nearness together of these sonorous particles at the commencement of their radiation, which also necessarily determines their comparative nearness together at any particular distance from their source.

It is perfectly evident, if sound consists of substantial corpuscles, as my hypothesis assumes, that a feeble sound at the start must be such because the sound-particles generated are few in number and consequently scattering, so that but a small number can enter the aural passage even when the ear is held near the sounding body; whereas, a loud sound at the commencement, or near the sound-producing instrument, is exactly the converse of this: the sonorous particles are densely compacted because a greater quantity is generated, owing to the molecular action which produces them being more effective or productive; and hence, in radiating and separating as the square of the distance from their source, they can necessarily pass to a considerable distance without being sufficiently thinned out or separated to appreciably weaken their effect on the sensitive membrane of the ear.

But carrying the idea still further, the most densely compacted mass of sound-corpuscles which may be supposed to collect about the mouth of a powerful steam siren will nevertheless, at the proper distance from it, produce a feeble tone, owing to the particles becoming so sparce or widely separated that but few of them can enter the ear at one time, and can thus produce but slight effect upon the tympanic membrane,—till finally, at a sufficient distance from their source, the particles will necessarily have become so separated and distributed over the continually augmenting mass of air that even if the auricular passage is not missed entirely a sufficient number can not enter it to affect audition, unless they should be converged into the ear by some kind of a funnel-shaped device such as an ear-trumpet. (See page 123.) Notwithstanding this extreme limit of audibility and apparent termination of the sound, it is easy conceiving, as every way probable, that all the original corpuscles, which produced such an intense effect near the instrument, may, as just remarked, continue to pursue their course through the air at their normal velocity, still more widely separating as the square of the distance, and not cease their journey till they have reached the extreme limits of the atmosphere.

This corpuscular hypothesis involves even more than has yet been explained. In addition to this weakening of the intensity of sound as the distance increases from its source, in consequence of the sonorous particles becoming sparcer or scattered by radiation over a wider and wider range of atmosphere, it is even conceivable that the corpuscles themselves may be larger or more massive in one case than in another, and that each sound-particle may itself be susceptible of becoming subdivided almost to infinity by giving off scintillations of its sonorous substance in all directions while passing through the air, the same as meteors have often been seen to do.

Thus, a feeble sound at the start, as in the tone of a *mosquito* or of a *bee*, may depend for its extreme faintness on the finer or smaller grade of sonorous corpuscles thus generated as well as on their fewness in number, which, supposing the corpuscular hypothesis true, would seem to be not only probable but reasonable.

Added to this, I have no hesitancy in believing that as a sound-pulse advances the gradual weakening of its tone (instead of being a less and less *motion of the air* as the wave-theory teaches, and which has been shown to be infinitely impossible by the singing of a locust,) may be and probably is due to the decrease in size as well as number of the sonorous atoms which constitute the sound and enter the ear.

I may even assume, in connection with the secondary or scintillating radiations of which I have spoken, the rational probability that the primary streams of sound-corpuscles as they leave the instrument may even emit a number of delicate secondary systems of sonorous particles in periodic pulses of distinctly different and *more rapid* vibratory rates, each system maintaining at the same time a relative concordant periodicity to the primary system of corpuscles,—while two instruments sounding together in the relation of some proper chord, as *third* or *fifth*, might even generate another and independent system of periodic pulses of a *slower vibratory rate* than either of the primary systems! This may not at present be intelligible to the reader, but I throw out the bare statement of the hypothesis here, as I shall revert to it before the close of this chapter in connection with another feature of the wave-theory which will beautifully illustrate what is here but darkly hinted. I hope, therefore, in view of its important future application that the reader will carefully re-peruse this paragraph before passing on, that it may be well impressed on the memory. I will only add here, if it be true at all that sound is constituted of substantial sonorous particles, then the secondary systems of radiating corpuscles, which I have assumed, if needed to explain the various phenomena of sound, would be neither insupposable nor improbable.

The truth is, the novelty of the corpuscular hypothesis constitutes the principal objection to its acceptance. We have been so constantly through life habituated to consider nothing as substance unless corporeally tangible that the mind naturally hesitates in conceding the substantivity of anything which eludes the senses as palpable material, or which will not submit to chemical analysis. But the world is growing, and despite the efforts of would-be science to keep it in its swaddling-clothes, seems destined to grow on till its present scientific raiment shall not only have become too small for it, but shall have also become so ludicrously threadbare and rent that true philosophy and science will be ashamed to look upon its semi-nudeness. In view of this encouraging tendency of the world to grow instead of retrograde, the writer proposes in a humble way not only to add what he can to the fertilizing and fructifying elements which may tend to accelerate its growth, but to lend a sartorial hand from time to time in helping to replenish its now scanty and tattered scientific wardrobe.

Returning to the assumption of sonorous corpuscles as the true solution of sound-propagation, it is easy to conceive the idea that at ten miles away from the steam siren, for example, we hear its sound faintly, not only because the sound-particles have become so scattered that only a few of them can enter the ear, but also because what few of them do enter have become so reduced in size by the constant emission of secondary radiations during their journey that they make but a slight impression on the tympanic membrane,—while we also hear the sound of the *gnat*, at a distance of only *six inches* from it, on precisely the same principle and for the same reason. In both cases the number and size of the sound-corpuscles, coming in contact with the sensitive membrane of the ear, determine the intensity of the tone; and the reason why we hear the sound of the *midge* as feebly at a distance of six inches as we do that of a *steam siren* at a distance of ten miles, is because the midge generates sonorous particles in number and size as much less than those produced by the siren as six inches are less than ten miles! Can any hypothetic solution of a scientific problem be more beautifully simple and consistent than this? And does not this view of sonorous propagation appeal for its probable correctness to the intelligence and scientific intuition of the reader? By the side of it, viewed only as a provisional hypothesis, I venture to assert that the supposition of an all-pervading *ether* as being a real substance circulating freely among the molecules of the diamond, which is now universally accepted by scientists, would be at once rejected as improbable were the two hypotheses submitted with their claims side by side to a competent and judicial scientific mind,—that is, on the supposition that both were equally novel. While this hypothetic *ether* is admittedly not known to exist by any scientific experiment or chemical process, it is at the same time wholly useless in Nature and in science, since every phenomenon occurring in *light*, as shown in the fourth chapter of this book, can be more readily explained by supposing the light-corpuscles themselves, in being propagated through space, to take the form of *waves* or *pulses*, than to ignore their existence by substituting this secondary substance (luminiferous ether) to be thrown into undulations, which but duplicates the mystery rather than simplifying the problem.

Not so, however, with these hypothetic sound-corpuscles. Although it is true that they can not be demonstrated to exist by direct scientific experiment or chemical analysis any more than can this so-called *luminiferous ether*,—standing thus far on an equal footing,—yet, as has been abundantly shown, while they meet every conceivable difficulty encountered, they are the only imaginable means left for explaining sonorous generation and propagation if the wave-theory breaks down, as break down it must, and consequently without recognizing the presence of such substantial sonorous pulses sound-phenomena must forever go without solution. I do not think I shall be charged with undue self-confidence or egotism in expressing the conviction that during the preceding arguments air-waves have been demonstrably shown to be inadequate to meet this case or to account satisfactorily for the hearing of sound at a distance. I need only remind the reader, as a proof of this statement, of the astounding fact of an insect converting four cubic miles of air into "condensations and rarefactions," with sufficient heat generated by the motion of its legs to add "one sixth" to the velocity of sound,—requiring, as was mathematically shown, thousands of millions of tons pressure,—to justify all I can say as to the utter insufficiency of the wave-hypothesis. Hence, the actual existence of substantial sonorous corpuscles, though of almost infinite tenuity, becomes a necessity of science, and thus solves the problems of sound generation and propagation by the exclusion of wave-motion, the only other conceivable hypothesis.

By the foregoing illustrations it can now be readily comprehended, on the supposition of a sound-pulse being constituted of substantial particles, how the entire range of the sound of a *gnat*, for example, may be confined within a single foot, though its sonorous corpuscles are radiated in the same manner, propagated at the same velocity, and governed by the same law of decrease in intensity, as are the sonorous discharges emitted from a steam siren. Both are controlled by the same law of decrease—as the square of the distance from the source—when properly understood. The sound-particles from the midge scatter and diffuse themselves throughout their limited range, becoming sparcer and sparcer, the same exactly as do those from the steam siren, while the intensity of its sound decreases from its greatest audibility to nothing within this trifling circumscription, just because the corpuscles being small in size and few in number become so reduced in bulk and widely separated within a single foot that a sufficient number can not concentrate within the aural passage to sensibly act on the auditory nerve.

In contrast with this simple and beautiful eclaircissement we have only to juxtaposit the wave-hypothesis by assuming that the tiny midge throws the air into physical waves constituted of "condensations and rarefactions," each one of which so compresses the air as to generate heat sufficient to add one sixth to the velocity of its sound, and the difference between the two solutions as to their probable correctness scarcely needs an argument.

Thus, while the beauty and consistency of this solution of sonorous propagation can hardly fail to meet the requirements of science, so far at least as beauty and consistency go, the new hypothesis also agrees admirably with other well-known natural phenomena resulting from the radiation and diffusion of substantial corpuscles, and in connection with which no kind of wave-motion of the air or of any other substance has ever been suggested.

Take, for example, a small rubber balloon filled with some kind of highly pungent odor, which, on being liberated in a still room of sufficient size, will furnish a complete illustration of the manner in which substantial sound-corpuscles may be supposed to radiate. Though controlled by a different law of conduction and traveling with a different velocity, yet the odor on being discharged will at once commence to propagate itself from particle to particle of the atmosphere and at considerable velocity, extending over a wider and wider range, and, as in the case of the diffusion of sonorous corpuscles, the fragrance will become less and less pungent as the square of the distance from the odorous center, growing weaker exactly in the ratio as the particles of the perfume scatter and become sparcer, by which means fewer fragrant corpuscles come into contact with the sensitive olfactory nerves.

Thus Nature has furnished us with a "mode of motion" which all science acknowledges to be constituted of real substantial corpuscles, though of such incomprehensible tenuity as to utterly baffle the imagination in attempting to conceive of them as substance at all, as was so fully illustrated by the hound and the fox. (See page 135.)

As intimated in another chapter, physicists have shown a want of shrewdness and business sagacity almost unparalleled in ever admitting odor to be a substantial entity, unless they wished to cripple the wave-theory of sound, since it is clearly susceptible of solution by means of some sort of hypothetic *odoriferous ether* which could easily have been invented, and which might assume the form of undulations as the air is drawn into the nostrils! What an oversight in physicists, that they did not think of it! There are really more good reasons, when we come to look at it, to be urged in favor of wave-motion in the case of odor than in the case of sound, since it is always connected with and accompanied by a rippling stream of air passing into the nose, whereas no such a plausible argument can be adduced in favor of undulations entering either the ear or the eye, since they have no basis in a stream of air or of any other substance moving along the aural passage, or pouring through the opening of the iris.

The radiations of sound-corpuscles and the decrease in loudness as the square of the distance from the sounding body, are governed by the same ratio precisely as shown in the case of *light*. In either case the decrease in intensity results from the same cause—the separation of the corpuscles over a wider and continually augmenting range of atmosphere. The reason why a carbon point, when intensely heated, as in a Drummond light, can be seen so much farther than the light of a candle-wick of the same size, is because the one generates a vastly greater number of luminous corpuscles than can be produced by the other, and possibly corpuscles of a larger size. And although the luminous atoms radiate in the same manner in all directions as do the corpuscles of sound, becoming sparcer and sparcer the farther they advance, according to this law,—*as the square of the distance from the source*,—yet the particles of light being more compact and vastly more numerous at the carbon point than at the candle-wick, it requires but the mental effort of a child to comprehend that at a definite distance—say a quarter of a mile away—the light of the candle might scarcely be visible, because its particles being fewer in number at the start would necessarily become more diffused and less in number in the space occupied by the eye, and consequently a less number of light-corpuscles would strike the retina; whereas the luminous atoms generated by the carbon point, being greater in number and more densely compact at the start are necessarily not so sparcely scattered at any single point a quarter of a mile distant, and hence a greater number would enter the eye and affect the retina at that station, and thereby cause the carbon light to appear the brighter. What possible solution of these wonderful phenomena, based on the undulatory movement of an all-pervading "*ether*," can be so beautifully consistent and clear?

But here a marked difference in the propagation of light and sound comes to the surface, which alone refutes the idea of both being wave-motion, even if one is, for the reason that the *waves of ether* and the *waves of air* should produce at least analogous results, since both are substances according to science, so called. Instead of being alike, their action is so obviously unlike and opposite that the judgment of every unbiassed mind, on observing the difference about to be pointed out, would at once decide that if one was wave-motion the other could not be.

I refer to the patent fact that sound can be heard even with one ear closed and the open ear turned directly away from the sounding body, and even when shielded from it by a large obstructing surface like that of a building, though, of course, the sound is not so distinctly heard as if the ear opened directly toward the sonorific body and without any intervening impediment; whereas light can not swerve to the right or to the left the smallest fraction of an inch, and can not be seen at all, even in the slightest degree, unless it enters the eye in a *direct line* either from the luminous body or from some reflecting surface.

If *air-waves* can lap around the head and enter the ear on exactly the opposite side, then *ether-waves*—if there is such an all-pervading substance as *ether*, and if there is any truth in the undulatory theory of light—should do the same thing, and thus enable us to see a candle at a distance in a dark night with the back of the head turned directly toward it! The two results are thus so diametrically opposite that the supposed wave-motion of two perfectly analogous substances—*air* and *ether*—can not explain both.

Even in the case of sound it is impossible to account for the phenomena of hearing, when the ear is turned directly away from the sounding body, by the supposed dashing of air-waves, as is clearly shown in the case of water-waves, and the complete protection afforded against their effects behind a projecting rock even of small dimensions. By means of such a rock that portion of the wave striking it is utterly broken and destroyed; and if any agitation of the water takes place behind the rock it is not the original wave which laps around the rock at all, but an irregular secondary or resultant tremor caused by the crispations of the water to the right and left produced by the broken ends of the passing waves.

Sound, however, acts in no such a way, and therefore can not be the result of wave-motion. If the listener is screened by an impenetrable wall, for example, or a building, the sound passes around it and enters the ear in its perfect form both as to pitch and quality, being only reduced in intensity; and if it consists simply of air-waves, as the current theory teaches, then these waves, unlike those of water, can lap around the building, enter the ear at an exactly opposite direction, and retain their perfect form and outline, though broken, distorted, and stopped by the obstruction, which is clearly an impossibility.

This single fact that sound is perfectly unbroken or undistorted, retaining its quality and pitch absolutely when the listener is stationed directly behind an obstructing wall, while a water-wave is completely shattered and destroyed by an obstructing rock without any power of inflecting around it, alone condemns the atmospheric wave-theory of sound, since every physicist who has written on the subject tells us that water-waves and atmospheric sound-waves are *exactly alike.* I do not exaggerate by italicising the last two words of the preceding sentence. A single citation from Professor Helmholtz, the leading physical investigator of Germany, will fully sustain this assertion:—

"The *process in the air is essentially identical with that on the surface of water.* . . . The process which goes on in the atmospheric ocean about us is of a *precisely similar nature.* . . . The *waves of air* proceeding from a *sounding body* transport the tremor to the human ear *exactly in the same way as the water transports the tremor produced by the stone.*"—*Sensations of Tone*, pp. 14, 15.

Hence, as the action of a sound-pulse is thus proved to be entirely different from the action of a water-wave,—the one retaining its perfect form and symmetry after passing an obstruction, while the other is entirely broken and obliterated,—it becomes a scientific demonstration that sound is not constituted of air-waves at all, nor propagated by means of them, since this highest living authority assures us that they are "essentially identical," "precisely similar," and act "exactly in the same way" as water-waves! This alone breaks down the wave-theory, if there was not another argument against it.

But the reader asks: "Does not this objection against the possibility of sound consisting of wave-motion, because it can inflect around an obstruction, militate with equal force against your own hypothesis of corpuscular emissions? If air-waves can not inflect, passing, for example, around a building, and thus enter an ear turned in an opposite direction, as would seem to be the case judging from the action of water-waves, how can sonorous corpuscles, radiating from a sounding body in straight lines, pass around a building and enter an ear under precisely similar circumstances?"

This would, at first sight, seem to be a serious objection to the corpuscular hypothesis; and, unless susceptible of being fairly explained, would be alone sufficient to condemn it.

While this perfect facility with which sound inflects, passing around intervening obstacles, necessarily overthrows the wave-theory,—based, as it is, on the undulations of a corporeal substance like our atmosphere, and acting in all respects like water-waves, I will now try to show that it does not necessarily break down nor even weaken the assumption of substantial sonorous discharges, constituted, as I assume, of radiating corpuscles.

It is easily conceivable that the particles of an incorporeal substance (if such substances can really exist, of which I have elsewhere given, as I consider, ample proof,) may not only radiate in direct lines, but, as recently intimated, may throw off secondary corpuscles in the form of scintillations, and that these again may radiate other and still lesser corpuscles, each system of which would be governed by the same law of diffusion and conduction, and thus travel through the conducting medium at a velocity exactly uniform with that of the primary corpuscles.

By means of such a subdivision of the original corpuscles of sound while they are being propagated through the air, with the secondary systems of lesser particles radiating in all conceivable directions, it is not only supposable and possible for such offshooting systems of corpuscular emissions to completely permeate the air on the opposite side of any obstructing object, but it rationally and philosophically accounts, at the same time, for the weakening of the intensity of sound under such circumstances, just about to the extent universally observed, while maintaining the pitch and quality of the fundamental tone unimpaired, as will be hereafter explained, which can not be predicated of wave-motion with the undulations, which are supposed to give shape to the sound, broken and distorted as they necessarily must be after striking an impediment which crosses their path.

We can thus not only imagine the primary lines of corpuscles darting away from the sounding body in infinite numbers, but can mentally see each of these original particles becoming itself a separate center of sonorous radiation, and by thus watching its progress can see it continually emitting, as it travels through the air, these secondary systems of corpuscles, while these in turn give birth to a third, these to a fourth, and so on *ad infinitum* so far as human imagination can follow them! By these secondary systems of corpuscles generating other offshooting systems, each constituted of smaller and smaller particles and all succeeding each other with such relative periodicity to the primary system of corpuscles as shall maintain the characteristic quality of the fundamental tone (to be fully explained at the close of this chapter), it is not at all difficult to see that the air may be permeated with sound throughout its most labyrinthian meanderings, the corpuscles passing by means of these succeeding secondary radiations over and around all kinds of obstructions, while, as before explained, the diminution of intensity would seem to exactly correspond to such superadded but constantly weakening corpuscular radiations.

Thus, while this hypothesis answers the purpose, fully accounting for the hearing of sound directly behind an obstructing wall, it remains an unanswerable fact that there is a spot in the water behind every obstructing rock of any considerable size at which no movement whatever of the interrupted waves can be perceived, even if we admit that such waves may partially lap around the rock and cause irregular crispations inside of the direct line of their course, which, as we see in the case of the supposed waves of *ether*, they can not and do not do in the slightest degree. Even, therefore, admitting this objection to be a possible difficulty in the way of the corpuscular theory, the weight of evidence is clearly against the wave-hypothesis, since the compound systems of radiating corpuscles will meet the case with a rational solution, while wave-motion will not meet it at all.

But the reader may ask, how about light? If sound can inflect and be heard distinctly behind an obstructing wall, why should not *light?* And why should any opaque body produce a shadow, since there can be no complete shadow in the case of sound? I answer that while my hypothesis of secondary corpuscular radiations explains the phenomena of sound, accounting satisfactorily for its power of inflection and its corresponding diminution of intensity after being thus inflected, *light* does not require any explanation of this kind at all. No solution of the sort is necessary, because light does not inflect, and therefore needs no solution on my theory to show why it does not. I have only to assume, as observation shows, that as a ray of light, passing through the air, is invisible at right angles, hence its corpuscles are devoid of secondary radiative power, and that this evidently is the reason why it can not bend around an obstructing body. While, therefore, I do not need to explain light at all, to adapt it to the hypothesis of corpuscular radiations, the wave-theory does need to explain both light and sound, since the action of sound, by inflecting without being distorted or marred, flatly contradicts wave-motion as seen in water, while light, by being devoid of inflective power, flatly contradicts sound, by showing that it can not be wave-motion if sound is. My solution of the difference between light and sound teaches that while sonorous corpuscles in passing through the atmosphere have this peculiar power of radiating secondary systems of corpuscular emissions, thus enabling sound to inflect and fill its proper place in the polity of Nature, light-corpuscles have no such radiative power, and do not need it, filling up their mission by their wonderful power of reflection. Hence, there is no inflection in the case of light. This natural difference between light and sound corpuscles is no more anomalous or surprising than the well-known fact that sound will freely pass through wood, which is entirely impervious to light, while both light and sound will pass through glass, which is a perfect bar to the corpuscles of electricity!

Before returning to the main question

I must not neglect to point out the superficiality, not to say absolute fallacy, of this *ratio of decrease* in sound-intensity, *as the square of the distance from its source*, laid down by Professor Tyndall, to which I have already incidentally referred, and on which the novel calculations recently made touching sound-velocity were based. But in exposing this fallacy and thus being obliged to show that this eminent physicist has unconsciously perpetrated one of the most glaring and astonishing scientific errors on record, it is nothing against him, individually considered, since every authority who has written on sound, light, or heat, including Professors Helmholtz and Mayer, assumes the same view and reasons from the same erroneous basis of calculation. It will take but a few paragraphs to expose and correct this fundamental error in science, assumed as it is in all works on natural philosophy, and thus show the reader what kind of so-called scientific information is being sown broadcast through the land for the enlightenment of our college students, and also to what kind of scientific instructors we are expected to look for accurate views of philosophy.

I now ask by what scientific authority does Professor Tyndall adopt "*feet*" *as the measure in estimating this ratio of decrease* in the loudness of sound? The reader has not forgotten his language, recently quoted. He says:—

"If its radius be *three feet* it will contain *nine times* the quantity of matter; if *four feet* it will contain *sixteen times* the quantity of matter, and so on. . . . The *intensity or loudness of sound diminishes in the same proportion.*"

Why did not this careful physicist, if he is as careful as he is reputed to be, adopt *meters*, or *rods*, or *inches*, or *furlongs*, or *miles*, or *leagues*, as his *measure*, instead of "feet"? Possibly we shall find out the reason after a little. Had he employed *rods*, for example, as his *measure* for determining this decrease in loudness as the square of the distance from the sounding body, in the place of *feet*, we would find the sound of the steam siren at a distance of ten miles diminished in loudness only the one 10,000,000th instead of the one 2,000,000,000th, as recently seen to be the case when "feet" were employed as the measure; and would thus have approached just two hundred times nearer to the truth, since the supposition of any sound being distinctly audible after being reduced to the one 2,000,000,000th of its normal intensity, is so preposterous that it only needs to be stated to be refuted.

But suppose, instead of *feet* or *rods*, Professor Tyndall had accidentally stumbled upon *inches* as his measure, which, if he had made it the subject of thought at all, he had exactly the same right to adopt. His language would then have read like this:—

"If its radius be three *inches* [from the center of the explosion] it will contain *nine times* the quantity of matter; if four *inches* it will contain *sixteen times* the quantity of matter, and so on. . . . The *intensity or loudness of sound diminishes in the same proportion.*"

It would really seem that had this scientist accidentally written *inches* instead of "*feet*," while preparing his lecture, he would have at once seen the infinite nonsense of the whole formula, and would thus have overthrown his ratio while he was writing it out.

Let us suppose the sound of the steam siren to diminish for ten miles as the square of the distance from the sounding body, and that we hold Professor Tyndall rigidly to the correctness of his mode of computing the ratio of proportionate decrease by compelling him to employ *inches* instead of "feet" as his measure. Then, instead of finding the sound at the ten-mile station possessing the one 2,000,000,000th of its original intensity, as it necessarily must have when "feet" are employed, it actually possesses but the one 400,000,000,000th as much intensity as at the start, or, in other words, it is but the one 200th as loud as it would be by adopting "feet" as the measure! Of course Professor Tyndall never thought of this, and I have no doubt the idea that it makes the least difference what measure is employed in determining this proportionate decrease in the intensity of sound, will be news to him! If it is not news to him, then he manifestly practiced an imposition upon his audience.

Now I will not here deny but that sound may diminish in loudness as the square of the distance from its source, under some sort of restricted measurement. But I ask, *As the square of what distance?* Surely not necessarily the same measure of distance employed in determining the quantity of air contained in a shell of a given thickness and at a given radius! Professor Tyndall sees no distinction here; but after correctly determining the quantity of matter in the various shells of air as the square of the distance, making it at 2 feet 4 times the quantity; at 3 feet 9 times the quantity; at 4 feet 16 times the quantity, "and so on," he adds: "*the intensity or loudness of sound diminishes in the same proportion.*" Yet we see by applying his measure of "feet" to the sound of the siren for a distance of ten miles we get one result, making the intensity decrease 2,000,000,000 times, while by applying *inches*, which we have the same right to do, we get an entirely different result, making the intensity decrease 400,000,000,000 times in the same distance! Surely both are not correct, while it is no doubt evident, even to Professor Tyndall by this time, that neither of them can be.

Perhaps we may aid this learned physicist by helping him to a simple rule for determining this ratio of decrease in the intensity of sound. In the first place, we may state it as a truism which no one will question, *that the measure to be employed in computing such proportional decrease in the intensity of particular sounds, if we estimate by the square of the distance at all, must always and of necessity vary exactly in proportion to the intensity of the different sounds at the start, or, in other words, as the range of the different sounds varies!*

Thus, for example, a very soft or feeble sound, though it may decrease according to this law, as the square of the distance from its source, till it becomes entirely inaudible, the same exactly as a loud sound diminishes, yet manifestly the measure to be employed in estimating its comparative decrease must be small in proportion to that of a loud sound. Instead of feet, meters, rods, or furlongs, in such a case it might require inches, quarter inches, or even lines, to get the proper result. Another sound of greater range, or of greater intensity at the start, might have its proportionate decrease in intensity approximately computed by employing "feet" as the measure,—while a very loud sound, such as that of the steam siren, having a range of ten miles, would evidently require a long measure to even approximate the true proportion. The superficiality, in a case of this kind, of using "feet" as the measure of computing the decrease, which Professor Tyndall makes alike applicable to the intensity of all sounds, without any discrimination, has been fully shown.

Let us now suppose the measure suitable for a sound having the range of the steam siren to be *half miles* instead of *feet* or *inches*. The statement of its ratio of decrease in loudness would then read something like this: At *two half miles* from the instrument the intensity of the sound would be but *one fourth* what it is as it leaves the siren; at *three half miles* the intensity would be but *one ninth;* at *four half miles* the intensity would be but *one sixteenth*, and so on; and at *twenty half miles* the intensity would have diminished, by such a measure of ratio, to *one four hundredth* of what it was at the start, which would manifestly approximate the correct proportion of decrease at that distance, instead of putting it at the preposterous reduction of one 2,000,000,000th of its original intensity, as the accidental measure of this eminent authority would necessarily make it.

I say *accidental*, because it is entirely certain, in reading his statement of this law governing such ratio of decrease in loudness "as the square of the distance," already quoted, that he had not the most remote idea that it would make any difference what measure was employed in computing such comparative decrease,—supposing, as any one can see by reading his statement, that the result would be exactly the same whether he used *miles*, *rods*, *feet*, or *inches*, or otherwise he would surely never have employed "feet" without some sort of qualification as to the range of the sound to be taken into account, thus committing himself, as he has done, to a fallacy in science of which he will be ashamed as long as he lives.

As a proof that this view of the matter is correct, it is evident if Professor Tyndall had been explaining the decrease in the intensity of light, as the square of the distance from the *sun*, he would never have used "feet" as the measure! Why? Because he would have intuitively felt, possibly without asking the reason why, that a mathematical progression based on so small a measure for such an enormous distance would have been simply ridiculous! Yet he tells us that,—

"The action of *sound* thus illustrated is *exactly the same as that of light and radiant heat.* They, like sound, are wave-motions. Like sound they diffuse themselves in space, *diminishing in intensity according to the same law."—Lectures on Sound*, p. 13.

In estimating the ratio of decrease in the intensity of the sun's light, as the square of the distance, this physicist would probably not think of using a less measure than *miles;* yet even this would be vastly too small to express the true ratio of decrease, as it would make the proportion of solar light on the earth but the one 9,000,000,000,000,000th of its intensity at the sun, which is an almost infinite exaggeration of the facts in the case. Instead of the measure for properly expressing this ratio being *miles*, if it were *million miles* it would be much more nearly correct, thus making the intensity of the sun's light on the earth but the one nine thousandth of what it actually is in contact with the photosphere of that luminary.

But the clearest demonstration of the superficiality of Professor Tyndall's use of "feet" in his ratio for determining the decrease in a sound's intensity (leaving us to infer that the same measure was applicable to all sounds) is the fact that *the entire range of many sounds is less than a foot!* The music of the midge, for example, as recently stated, is inaudible at the distance of a foot, though intensely audible if performed, as it often is, near the entrance to the auricular passage.

Now, this sound, like all others, decreases in loudness according to the same uniform law, call it "as the square of the distance from its source" if you like, to the extreme limit of its audibility, which it does as literally and truly as does the sound of a steam siren with its effective range of ten miles. Yet how laughably absurd it would be to apply Professor Tyndall's measure of "feet" to the music of these ephemera! Let us try it: If the distance from the midge be *two feet the loudness of the sound will be one fourth;* if the distance be *three feet the loudness will be one ninth;* if the distance be *four feet the loudness will be one sixteenth, and so on!* Yet the sound entirely ceases within a single foot, and thus passes through all the gradations of decrease "as the square of the distance;" and even through a greater progression of diminution within this foot than the sound of the fog-horn passes through in a range of ten miles, since it is still distinctly heard at that distance! To employ "feet," therefore, in computing the ratio of decrease in the loudness of the sound of a *gnat* would be a measure about as much out of proportion one way as it would be enormously too small when applied to the sound of the *steam siren.* It is therefore manifestly evident that these beautiful distinctions, equally applicable to decrease in the intensity of sound, light, and heat, which seem so self-evident that a schoolboy who had used a slate and pencil for a single month ought to have noted them, never entered the mind of this eminent lecturer, who is quoted as standard authority in physical science all over the land, and whose works on sound, light, and heat are so eagerly sought for by scientific students among all nations that they have been already translated into the principal languages of Europe!

It is thus seen that the amplification of the wave-theory at every turn, even in the hands of its ablest exponents, necessitates the employment of laws, formulas, and ratios, which, when analyzed, are found not only to be pitiably insufficient, but completely subversive of undeniable facts of science and well-known principles of mechanics.

Though I have been thus forced into a digression from the main argument based on the supposition of ' condensations and rarefactions," in order to explain the corpuscular hypothesis, and also to correct Professor Tyndall's misapprehension as to the proportional diminution of sound-intensity, thus reducing the decrease in the sound of a steam siren from one 2,000,000,000th of its intensity, according to his ratio, to about one 400th, still it does not weaken the argument drawn from such diminution, by which I showed a corresponding decrease or increase in sound-velocity. It only brings the fatal effect of the heat hypothesis of Laplace within the comprehension of the mathematician. It still remains an unanswerable fact, if there is any truth in the solution of Laplace or in the idea of "condensations and rarefactions" of the air produced by sound, that the velocity of sound and the loudness of sound must correspondingly increase and decrease together, since the augmentation of velocity depends upon the amount of heat generated, just as the heat depends upon the amount of the condensation, while *it is also upon the difference of density that loudness depends.* Hence, the heat solution of Laplace based on such condensations of the atmosphere must necessarily be a fallacy.

As all physicists will admit that this discrepancy of Newton overthrows the wave hypothesis unless it is susceptible of a satisfactory scientific explanation which will reconcile it with the observed velocity of sound, and since the heat solution of Laplace—the only one ever claimed to meet the difficulty—turns out to be not only no solution at all, but an unmitigated scientific excrescence, literally lugged into the theory to meet a desperate emergency,

may we not fairly conclude that, as the **question** now stands, the discrepancy of **Newton** still remains unimpaired, and consequently that the wave-theory now occupies the anomalous position of an edifice whose foundation is utterly shattered?

Even if the unanswerable difficulties in the way of this hypothesis of Laplace now being presented had never been named or thought of, the supposed *relation of density to elasticity* as the law which determines the velocity of sound through all bodies, and on the analysis of which Laplace formulated his solution, can be shown beyond all question to have no foundation in science or in fact, being purely chimerical, and contradicted by the observed velocity of sound through various well-known substances in addition to our atmosphere, so signally demonstrated in Newton's calculation to be in direct opposition to the law. This relation of the *density* of a body to its *elasticity* as the basis of sound-velocity through all bodies, like the wave-theory which it supports, is a mere hypothesis fabricated and formulated for a specific purpose out of a few superficial observations,—invented, in fact, to aid wave-motion by systematizing its principles, the bottom of which is shown, the moment it is held up to the light, to have fallen out in the time of Sir Isaac Newton. If there were nothing else to prove my assertion true, that single demonstration of Newton, in his careful analysis of the density and elasticity of the air, shows that this universal medium of sound-conduction is diametrically opposed to the hypothesis, unless aided by the heat solution of Laplace, which, when examined, turns out to be grotesquely impracticable, having been formulated, as just shown, without the shadow of science or reason to justify it, since there is neither condensation nor heat produced by sound.

At the time Newton made this discovery, physicists who advocated the wave-theory of sound appeared intuitively to agree among themselves that if this single discrepancy in their formula could, by hook or by crook, be reconciled, and the difficulty successfully explained away, their theoretic coast would be clear, and that all other bodies or substances whatever as sound-conductors could be readily made to fall into line and quiescently conform to this law of density and elasticity. Yet one would have supposed, after Newton had thus shown by undeniable figures and facts that this law of velocity was wrong as related to atmosphere, by a palpable discrepancy of 174 feet a second, that physicists would have weakened sufficiently at least to look around them and see if it were not possible for other bodies through which sound travels to show like indications of rebellion against their law. Instead of doing so, they bent all their energies to the one task of overcoming this single admitted contradiction of the wave-theory as based on the known elasticity and density of the air, making all sorts of ingenious suggestions without success, till at last the scientific mountain, having labored, brought forth this contemptibly small and ludicrously deformed mouse of Laplace!

Professor Tyndall briefly states this law of density and elasticity as applied to the air, which is equally applicable to all other kinds of sound-conductors, as follows:—

> "The velocity of sound in air depends on the *elasticity* of the air *in relation to its density*. The *greater* is the *elasticity the swifter is the propagation;* the *greater* the *density the slower is the propagation.*" —*Lectures on Sound*, p. 45.

Now, as a matter of course, if a body could be found having *great density* and *no elasticity*, it is clear, if there is any foundation for this law, sound should not travel through such a substance at all, since this is evidently what the law means if it means anything. Such a body we have in *lead.* It is not only among the *densest* of metals, but is almost entirely devoid of elasticity (as much so nearly as a mass of putty), according to every known definition of the term *elasticity* given in our dictionaries. Yet it is a fact, as admitted by Professor Tyndall himself (*Lectures on Sound*, p. 39), that sound travels through *lead* with a velocity of over 4,000 feet a second, or nearly four times its velocity in air! What, then, becomes of this formidable law based on the relation of density to elasticity?

I see no way for scientific investigators to get over this new *leaden* difficulty, unless some modern Laplace will invent another hypothesis, based, say, on the peculiar molecular structure of this metal, and show by some sort of an elaborate formula that a sufficient amount of *electricity* is generated by the passage of a sound-wave through it to counterpoise this lack of elasticity! Possibly the facility with which lead fuses might interfere somewhat with the generation of a sufficient electrical current to meet the conditions of the new hypothesis. At all events, it could be easily modified in half a dozen ways to make a much more plausible showing than the original Laplace made in adding 174 feet a second to the velocity of sound in air on the ground of the generation of heat by sonorous "condensations and rarefactions" which never had an existence, and never can have, except in the highly wrought fancy of physicists.

But supposing this formula to be adjusted to suit the molecular structure of *lead*, there would be a similar trouble at once with pure *gold* and *copper*, which are likewise practically devoid of elasticity, though they are among the densest of metals. Yet this same high authority assures us that sound actually travels through *gold* at a velocity of 5,000 feet a second, and through *copper* at a velocity of 11,000 feet, or ten times its velocity through the atmosphere, which is known to be among the *most elastic* and *least dense* of physical bodies! (See *Lectures on Sound*, p. 39.)

The truth is, this so-called "law" as the basis of sound-velocity, formulated on the *relation of density to elasticity*, is as fallacious as is the wave-theory built upon it, and the two hypotheses therefore are well matched, being equally destitute of scientific foundation. Hence, we are again brought around, almost unexpectedly, to the same great scientific and natural fact that sound travels through all bodies with a velocity and facility exactly commensurate with their conductive quality, whatever that may consist in, depending on molecular structure,—that is, the relative position and arrangement of their ultimate atoms, —and perhaps other conditions at present unknown, the same as those under which electricity travels and by which it is governed, though each acts under the control of laws peculiar to itself. No man can tell why electricity passes through *copper* or *silver* with greater facility than through *iron* or *platinum;* nor can any one formulate a law of elasticity, or density, or compressibility, or porosity, or ductility, or malleability, which will explain why electricity will not pass, for example, through glass at all, which is the best known conductor of sound, so far as velocity is concerned.

These laws of conduction, radiation, diffusion, attraction, repulsion, &c., as before remarked, are among the unknown, and, at present, unknowable mysteries of Nature. Whenever we shall accept the great fundamental truth that we are surrounded with substantial but incorporeal entities, such as light, heat, sound, electricity, magnetism, gravitation, &c., whose laws and principles of operation, as inscrutable as their author, lie hidden in the Ultimate Causation of all things, the relations of which, as well as their modes of operation, can only be apprehended by mortals in the contemplation of their corporeal results through experiment and observation, we shall then have arrived at a much better mental condition for the attainment of true scientific knowledge than by assuming pretentious laws and formulating elaborate hypotheses for the explication of the unsolvable mysteries of Nature, and which, as recently witnessed, contravene not only the unalterable decrees of mathematics, but render nugatory the stubborn facts of mechanics exemplified in the constant experience of every living creature.

When the discrepancy, of which I have been speaking, of 174 feet a second between the observed and the calculated velocity of sound, was first discovered by Sir Isaac Newton, he should have at once abandoned the undulatory theory of sound as a practical absurdity, contradicted in its fundamental principles by the observed facts of Nature, and thus have saved the world the demonstrated result not only of sound traveling at a velocity of 66,000,000 miles a second, as logically deduced in the case of the steam siren, but the infinitely impossible performance of an insect shaking four cubic miles of atmosphere into "condensations and rarefactions" by the movement of its tiny legs in the free air, thus exerting a mechanical force of 5,000,000,000 tons, according to the plainest vulgar fractions furnished by Professor Mayer, or 66,000,000,000,000 tons, as shown by the indisputable heat and pressure figures of Professor Tyndall made necessary by the solution of Laplace.

No! Instead of doing such a sensible thing as at once giving up the hypothesis as untenable, Newton took it for granted that nothing but the wave-theory would answer the purpose, or have any effect in solving the problems of sound, since it was at that time, as it is now, the universally accepted hypothesis; and hence he began to cast about for some sort of explanation of this discrepancy which might reconcile it with the observed velocity of sound, and which, as already seen, finally culminated in the enormous folly of Laplace's solution, involving the actual generation of heat, by the singing of a locust, sufficient to raise a full head of steam in *twelve hundred million locomotive-boilers at one time*, as any mathematician can calculate by transferring the heat thus generated in the condensed half of the air to the proper number of cubic feet of water!

A more astounding want of philosophical sagacity than was thus exhibited by Newton and his contemporaries in not giving up the wave-theory as a fallacy of science, after its foundation had been swept away, is not on record, and it will be so regarded by future physicists while books are read.

But here, unexpectedly, this locust can render me another little service by showing how easy it is for a false theory to contradict itself when it comes down to the discussion of details. I have already given numerous examples of this kind from the writings of these eminent physicists whom I have the honor of reviewing; but those are only mere specimens of what may yet be expected, and of which these works on sound are necessarily full from beginning to end. This is no exaggeration; for it is practically impossible for the ablest advocates of the theory, in writing an extended treatise on the subject, to discuss the details of one branch or one class of phenomena, without flatly contradicting the principles, ratios, and laws enunciated when treating on another, owing to the inherent incongruity necessarily subsisting between the different elements of every erroneous hypothesis. Of this the reader, if not already convinced, will be amply assured as the review progresses.

I now propose to prove, by Professor Tyndall himself, that this insect, which can be heard a mile in all directions, and which has been so provokingly used against the wave-theory, *can not by any possibility stir the air more than a few feet around it.* In doing so, it will be seen that it does not require the overwhelming mathematical arguments here being presented to shatter the hypothesis. I only need to let this most popular exponent of the theory speak out, as he plainly does in numerous places, and then array his language in proper order before the reader to annihilate the very foundation of the wave-hypothesis.

The reader no doubt remembers that when this lecturer was trying to explain to his audience the principles of resonance, and how it was that a sounding-board augmented the tone of a string (examined at page 82), he gave a demonstration of the well-known fact that a string stretched over rigid pieces of iron, unconnected with wood, produces no sensible effect upon the auditory nerve even half a dozen feet from it, however vigorously it may be caused to vibrate. He then undertakes to explain this to his audience, and the reason he assigns why we hear no sound is that a harp-string or piano-string is too "thin a body" to produce any "sensible" effect upon the "*air*"! As this argument on resonance is important, and conclusively wipes out the wave-theory when applied to the stridulation of the locust, I will requote his words consecutively, that the reader may not fail to see their force. He says:—

"It is not the *chords* of a harp, or a lute, or a piano, or a violin, *that throw the air into sonorous vibrations.* It is the large surfaces with which they are associated, and the air inclosed by these surfaces."—*Lectures on Sound*, p. 88.

I now ask Professor Tyndall why it is that the vibrating string, "swiftly advancing," as he says in another place, *carving* and *moulding* the air into "sonorous waves," and sending them off in the form of "condensations and rarefactions" at a velocity of 1120 feet a second, can not at this particular juncture "*throw the air into sonorous vibrations*" *at all?* He answers:—

"The *amount of motion communicated by a vibrating string to the air is too small to be perceived as sound even at a small distance from the string.*"

"The *sonorous waves which at present strike your ears* do not proceed immediately from the string. The *amount of motion* which *so thin a body imparts to the air is too small to be sensible at any distance.*"—*Lectures on Sound*, pp. 87, 125.

This suicidal admission establishes precisely what I have been all the time contending for since the commencement of this chapter, namely, that "so *thin* a body" as a *string* or a *tuning-fork*, especially with such a trifling aggregate velocity as only *seven or eight inches a second*, can not by any possibility drive air-waves even "*a small distance*" *from such string or fork!* Here it is unwittingly admitted to be true, since "the *amount* of *motion* which so *thin a body imparts to the air is too small to be sensible at any distance*"!

Notwithstanding these contradictory admissions, with which a schoolboy could overwhelm the undulatory theory, this great physicist teaches, as he is compelled to do unless he utterly renounces air-waves as the means of sound-propagation, that a *locust*, weighing not a *hundredth part* as much as a harp-string which produces the same tone, and having no strong man's fingers to pluck it, and thus "mould," "carve," and "send" off aerial undulations, is capable, while sitting on a green leaf, and without any "large surfaces" to act as sounding-boards, by the simple movement of its tiny, threadlike legs, of generating an atmospheric disturbance which fills four cubic miles with "condensations and rarefactions," the atmospheric pressure of which generates heat sufficient to add 174 feet a second to the normal velocity of the sound throughout this vast area! Was there ever a more ridiculous position overthrown by a more maladroit and suicidal self-stultification?

Instead of physicists any longer teaching atmospheric wave-motion as the true cause of sound-propagation, let it now be proclaimed to the scientific world that this "highest living authority" on sound, as Professor Youmans designates him, in the most unmistakable language, has abandoned the wave-theory, and has admitted that a locust does not and can not produce its wondrous stridulation, heard a mile in all directions, by means of air-waves, unless he shall publicly repudiate his statements just quoted, namely, that "The *amount of motion which so thin a body imparts to the air is too small to be sensible at any distance*," or "*too small to be perceived as sound even at a small distance from the string*" or insect!

He surely will not pretend to claim, after these reiterated and voluntary statements, —admissions of facts in regard to the string and its limited tone which are patent, undeniable, and unavoidable, on his part,—that the *legs* of a locust can produce any more effect on the air than can a harp-chord of a hundred times the size and a thousand times the weight If not, what then becomes of the helpless wave-theory, deserted by its best friend and ablest defender? If he utters the truth in what he here says, and repeats in different forms in regard to a powerful sonometer-string, namely, that "the amount of *motion* which *so thin a body imparts to the air is too small to be sensible at any distance*," and "*too small to be perceived as sound* ☞ *even at a small distance from the string*," can it possibly be true, or anything short of an unmitigated falsification of science and fact, when he teaches, as he is obliged to do unless he renounces the wave-theory, that the *legs* of an insect, moved with less than a thousandth part of the *vis viva* applied to the string, actually hurls the air into waves which are "*perceived as sound*" a mile away, and which fills *four square miles with "sensible" sonorous pulses?* And, finally, has not Professor Tyndall flatly admitted that the sound of this insect is not and can not be produced by any undulatory movement of the air possible to be produced by "so thin a body" as the legs of a locust? And if so, is it not an unconditional surrender of the wave-theory, and an unintended confession that the whole hypothesis is a pure fallacy of science? If this is not what his admissions amount to, under the most liberal construction, then I confess I have no correct understanding of the English language.

I now make the unqualified assertion, which I believe the unbiassed judgment of the reader can but approve, that there is not a man living competent to reason on any question of science, or qualified to draw a logical conclusion from established premises, who, with these admissions of Professor Tyndall as his guide, can believe it possible for a locust to stir a single cubic perch of atmosphere by the motion of its threadlike legs, to say nothing of its ability to churn into "condensations and rarefactions" four cubic miles of air, not only causing its particles, as Professor Mayer expresses it, to "swing to and fro with the motions of pendulums," but to generate sufficient heat to add "one sixth" to the velocity of its sound!

To suppose any man capable of believing, after the foregoing citations (supposing Professor Tyndall's views correct as to the effects on the air of a vibrating string), that so diminutive a creature as a locust can actually convert such a vast atmospheric area into "condensations and rarefactions," exerting a pressure sufficient to generate the heat involved in the hypothesis of Laplace, would be to suppose him hopelessly insane and mentally irresponsible for his acts.

The reader may now pertinently ask how it is possible that a pretended scientific theory, so utterly devoid of foundation in fact and so ridiculously absurd in reason and philosophy as the foregoing arguments appear to make this, should have continued to exist from generation to generation, and to be accepted as true science by the most enlightened and critical minds of the world, in all ages. Why, he may appropriately inquire, has not some one else, of all the thousands who have investigated this question, made the important discovery, if discovery it be, that the wave-theory is a baseless fallacy, with all these mechanical facts and fundamental considerations as open to examination and as susceptible of being understood by every other physicist as by the writer of this monograph?

I can only say, in reply to this natural inquiry, that the blinding effect of a universally accepted theory, however false and absurd, handed down from one generation to another, indorsed by the authority of the greatest intellects, and the tendency of such a theory to stifle doubt and paralyze critical investigation as to the foundation on which it rests, and thus to prevent the origination of any inquiry concerning its conflicting phenomena, except so far as to harmonize them with its admitted scientific basis, is one of the most singular, as well as one of the best established psychical facts in the history of intellectual progress.

The Ptolemaic theory of astronomy, which made the earth the center of the universe, and taught that the sun, moon, and stars revolved around it every twenty-four hours, and which had stood for two thousand years comparatively unchallenged, just because each preceding generation had passed it along to the next without calling its fundamental principles in question, though philosophers of every age, from the time of the Ptolemys down, had been terribly puzzled over its contradictory details, furnishes a vivid illustration of the tendency of any theory, which has existed for centuries, to close up, by the accumulating debris of ages, all the passages which at its commencement may have led to the subcellar and to its very foundation-walls.

This very difficulty, which so puzzles the reader, as to how it is possible for the wave-theory to have remained unshaken for so many generations, without a single physicist venturing to call it in question or expose its self-evident absurdities, and yet that it should be all the while false and without the least foundation in fact or science, was precisely the argument made use of in the time of Copernicus and Gallileo in favor of still continuing to adhere to the Ptolemaic hypothesis! Gallileo replied to this reasoning that the truth or falsity of the new hypothesis must be judged by the weight of facts and the force of mathematical deductions, and not by superficial appearances or the plea of authority based on what philosophers may have taught in ages past;—that some one had to be the first to discover any new scientific truth, and especially to find out the true relations existing between the earth and the other members of the solar system, and their relation one to another and that Copernicus, out of all the millions who had thought upon the subject, happened to combine the particular qualifications and to be trained with the proper educational advantages which enabled him to break through the film of false reasoning and to grasp the key which opened the door into the avenue leading to the true solution of the problem. The scientific conflict was severe; but the Copernican theory finally prevailed, and is now universally believed, notwithstanding the specious argument of the philosophers of that day based on this always unsafe criterion of venerated authority.

So, I predict, will the corpuscular hypothesis of sound finally triumph over the venerable wave-theory, without a tithe of the conflict or enduring doubt which characterized the decadence and final dissolution of the Ptolemaic system; and with no decree, civil or ecclesiastical, to check the outward strides of the one or bolster up the waning fortunes of the other. In this view I confidently look forward to the near future, when it will be as rare a circumstance for a physicist to express a belief in atmospheric waves as the true mode of sound-propagation, as it is now to hear any man pretending to a scientific education suggest the possibility of the earth being stationary and flat instead of being a revolving globe!

For an astronomer at this day to be obliged to reason with a pretended philosopher who could really assume, on account of mere appearances, that the earth necessarily stands still, and that the millions of celestial bodies actually revolve about it every twenty-four hours; and to be compelled to seriously go into the details of argument with such a mind, after knowing what an astronomical student must necessarily know about the motions of the heavenly bodies and the infinite impossibility of such a supposition being true;—and feeling, as he would be forced to feel, that a man pretending to the least degree of scientific education must be absolutely without excuse for holding to so stupid an idea in this age of general intelligence, requires about the same degree of patient equanimity of temper, and shows a parallel example of the mingled commiseration and astonishment which the writer of this review is compelled to cultivate and to feel while patiently pointing out the self-evident fallacies and inconsistencies of the wave-theory of sound, and the pitiable involvement of these eminent scientific investigators who are so misguided and self-deceived as to advocate it.

Should any physicist a hundred years hence happen to be so illy informed and so far behind the age as to believe in and advocate the preposterous positions involved in the current wave-theory of sound, the educated scientist of that epoch in attempting to set him right will then feel about the same indefinable sensation of pity mingled with disgust that the astronomer of to-day feels when hearing some scientific lunatic urge, as is sometimes the case, that the earth can not revolve on its axis, because, if it did so, it would overturn the water-bucket; or that the writer of this review is compelled to feel while trying to convince Professors Tyndall, Helmholtz, and Mayer that a locust can not, by moving its legs, throw four cubic miles of air into "condensations and rarefactions," and thus exert a mechanical pressure of thousands of millions of tons!

The lesson taught us by the humiliating fact of the long-enduring sway of the Ptolemaic system of astronomy, while all the time absurdly false, should warn us against taking anything in science on trust, or believing it to be true just because it is sanctified by the indorsement of a long and immortal line of scientific names,—especially while anything about it has not been subjected to the most scrutinizing scientific research. The creed to which I have sworn fidelity, and to which I have affixed my hand and signet, though a negative creed, is nevertheless my Bible in all scientific matters, namely, not to accept anything as philosophical or scientific truth, or to allow it the weight of a feather in my convictions, because it has been believed in or advocated by any man or set of men, however renowned their names may be.

A pet bear, it is said, can be so long accustomed to being chained to a stake that it will continue on to circle in the same beaten path without thinking of venturing beyond the limits of its wont, even for days after its chain has been removed. There have been scientific pet bears in all ages, and I fear the race has not yet become extinct.

An illustration of the force of habit and the influence of traditional authority handed down from predecessors by which we are many times led to accept the greatest of absurdities without calling them in question, is given in a story told of a certain commandant of an old fortification somewhere I think in Germany, who, on assuming command of the station, found that every morning and evening, as regularly as the sun rose and set, a soldier was stationed as guard, by the subordinate officer, over a certain piece of ground near the mote. The commandant, though struck with the circumstance, supposed it to be all right, and therefore did not require an explanation, but proceeded to attend to his daily routine of duties. At length, continuing to observe day after day this singular and apparently uncalled-for changing of guard, he concluded to inquire the cause of so strange a custom. But on questioning his staff-officers they were unable to give him any information on the subject. He then called up an old sergeant who had been stationed at the fort for many years, but his inquiries met with the same result. The sergeant informed his superior that when he came there it was customary to place a guard over that piece of ground every morning and evening, and that the sergeant who had preceded him for years told him, on being transferred, that it had been the custom since his first entrance into the service.

At last the commandant began an examination of the records kept by his predecessors, when, finally, to his astonishment, he ascertained that forty years previously a certain officer in charge of this fort had brought his family to reside with him during the summer,—that, for their comfort and convenience he had planted this patch of ground with *cabbages*, and that some neighboring *cows* being in the habit of breaking into his garden through the frail fence, he had deemed it expedient to station a guard to keep them away! But notwithstanding the neighboring farm-house, and with it the cows, had long since disappeared, and although no cabbages or other vegetables had been grown upon this spot of ground for forty years, yet the succeeding officers in charge, year after year, without inquiring into the reason why, but faithful to the traditions of their predecessors, and alone from the force of habit and out of respect to authority, had continued the practice of mounting guard over this vacant cabbage-patch because it was customary to do so!

In about the same manner, and for reasons not a whit better, Newton, Laplace, Helmholtz, Tyndall, and Mayer have continued year after year and generation after generation to place the wave-theory on duty just because the custom was inaugurated by Pythagoras 2,500 years ago, and wave-motion made to stand guard over one of his superficial observations,—while modern physicists, with their immeasurable scientific advantages, could have easily seen, had they exercised their reason and examined the records of Nature, that the *cows* and *cabbages* of that ancient philosopher, if they ever existed, have long ago disappeared, leaving no use whatever for the wave-theory of sound to be placed on guard.

But even yet I have not extracted my strongest and most conclusive argument out of that valuable *locust*, which has been stridulating so unpleasantly in the ears of physicists, and playing such tantalizing havoc with the wave-theory during so many pages of this chapter. I now have another service for it to perform, which will so completely overthrow the assumption of atmospheric sound-waves as apparently to end the controversy on the subject, and in such a way as would even seem not to admit the intervention of a quibble to save the hypothesis from destruction. I make this somewhat confident prefatory remark at introducing this argument in order to prepare the reader for what may be safely termed *demonstrative evidence* against the wave-theory, even if any ambiguity may have been imagined as attaching to previous arguments. I am willing, so far as the truth or falsity of the wave-hypothesis is concerned, to entirely ignore the preceding considerations, as if they had no existence, and let the theory stand or fall on the merits of the single argument now to be presented, to which I especially invite the attention of the three eminent authorities whose writings I have the honor of reviewing.

There is not a physicist, ancient or modern, who has written on sound, but teaches in unequivocal language that the *tympanic membrane* is actually *shaken* or caused to *vibrate* by sonorous pulses through the dashing of air-waves against it, driven off from the sonorific body; and that this vibration of the "drum-skin of the ear," as Professor Helmholtz terms it, swinging in synchronism with these beating waves, is the way we hear sound, and the only means by which sonorous impressions are conveyed to the auditory nerve, and through it carried to the brain, and there translated into the sensations of tone.

To the well-informed student of the physical sciences I would need to present no proof of a statement so universally verified by the writings of authorities treating on this subject; but I am writing for the unscientific masses as well, and shall therefore present a few concise extracts from Professors Tyndall and Helmholtz, that no reader shall say I assume the question to be proved. Professor Tyndall remarks:—

> "Thus is sound conveyed from particle to particle through the air. The particles which fill the *cavity of the ear are finally driven against the tympanic membrane*, which is stretched across the passage leading to the brain. This *membrane*, which closes the drum of the ear, *is thrown into vibration*, its *motion* is *transmitted to the ends of the auditory nerve*, and afterwards along the nerve to the brain, where the *vibrations are translated into sound*."
>
> "Thus, also, we send out sound through the air, and *shake the drum of the distant ear*."—*Lectures on Sound*, pp. 4, 5.

This language can not be misunderstood. There is nothing figurative, poetical, or ambiguous about it. He means by "vibrations" the actual displacement of the bending portion of this membrane, or its literal *oscillation, inward and outward*, as each successive air-wave strikes it. As a proof that such is his meaning, he repeats this fundamental doctrine of tympanic vibration in so many ways that we are left without any doubt on the subject. Take the following:—

"Imagine the first of a series of pulses which follow each other at regular intervals, *impinging upon the tympanic membrane. It is shaken by the shock;* and a body once *shaken* can not come instantaneously to *rest.*"—"Every *wave* generated by such *vibrations bends* the tympanic membrane *once in* and *once out.*"—*Lectures on Sound*, pp. 49, 69.

This, also, is concise and to the point. A sound, to reach the brain at all, and there be translated into its proper sensation, must do so by first acting on this drum-skin of the ear,—bending it "once in and once out" for "every wave generated." It matters not how *faint* this sound may be or at what *distance* away from its source it is heard; we only hear it by the oscillations of this membrane, if the wave-theory be true, for this great authority assures us that "we send out sound *through the air*, and *shake the drum of the distant ear.*"

Professor Helmholtz, who, as I have already hinted, stands first among all the authorities on sound, fully corroborates this view. In fact, he is the main source of authority from which Professor Tyndall and all minor writers on sound draw most of their inspirations. I will quote a sentence or two from him to show that his views correspond in every respect with those of Professor Tyndall:—

"A periodically oscillating sonorous body produces a *similar periodical motion, first in the mass of air and then in the drum of our ear, and the period of these vibrations must be the same as that of the vibration in the sounding body.*"—"We have already explained that the mass of air *which sets the tympanic membrane of the ear in motion,*" &c. —*Sensations of Tone*, pp. 16, 45.

I could quote hundreds of passages to the same effect from various authorities, including Professor Mayer, had I space to spare or were they necessary. I simply assert, as all scientists well know, that this is not only the uniform teaching of the current sound-theory, but it is the very foundation on which the wave-hypothesis rests, since it is perfectly manifest if the tympanic membrane does not vibrate in periodicity to aerial undulations that atmospheric sound-waves are wholly useless as the mode of sound-propagation.

This fundamental doctrine, therefore, of the vibratory motion of the tympanic membrane in response to sound, however feeble or at whatever distance from its source it may be heard, is vital to the wave-theory, and no physicist will hesitate a moment to admit that the two must stand or fall together. If, therefore, I shall be able in this argument to demonstrate that the tympanic membrane does not and can not vibrate at all in response to sound, and that it is not so intended to vibrate in the slighest degree, it is clear that the wave-theory falls to the ground. I first propose to demonstrate this by the stridulation of the locust.

First of all, this "drum-skin of the ear," it must be distinctly understood, is a physical, ponderable body, stretched across and closing the auricular passage, and hence must have a certain definite amount of *weight* or *inertia*, and must therefore necessarily require a definite and calculable amount of *mechanical force* to displace it, even if freely suspended in the air, to say nothing of the extra force which would be required to bend it "once in and once out" at every wave, and thus overcome its tensive resistance in addition to its weight. I shall at present only consider the question of *inertia;* and I care not how trifling that may be in the case of a single "drum-skin," it answers my purpose just as well, as the reader will soon see.

A single tympanic membrane can easily

be weighed on any druggist's scales, and the weight accurately ascertained and recorded. Take that portion of the membrane free to bend in and out by alternate external and internal pressure, and it is found to be equal to about a quarter of an inch square in superficial area, and averaging about a sixty-fourth of an inch thick. In order to meet this case with unquestioned facts and figures, I have taken the trouble to secure a perfect specimen of this membrane, though somewhat less in weight than in a living subject, and I find its actual weight to be a fraction over half a grain,—making, in round numbers, 16,000 of such drum-skins to the *pound* avoirdupois. Here, then, is a mathematical basis for arriving at definite mechanical results in regard to the physical strength of this locust, which can not be gainsaid or doubted.

In the next place, I have easily ascertained, as the reader can also do, that a single specimen of this "drum-skin" can be stretched within the equivalent space occupied by a cubic quarter-inch block, leaving an abundance of room on either side for it to vibrate to and fro by the action of sound, if it does ever so vibrate. We have, then, only to suppose one tympanic membrane accurately and sensitively located in the space of each cubic quarter-inch throughout the four cubic miles filled by the sound of the locust, and as certain as there is any truth in the wave-theory of sound, all these membranes must be thrown into vibratory motion, if stretched with the same tension as they are in human ears, because it is perfectly evident that an ear, if present at any quarter-inch throughout this mass of air, would hear the sound of the stridulation, which, according to this theory, could only occur by the *shaking* of this "drum-skin"!

Now, by a simple calculation, which any schoolboy can verify, I find that there is room enough in this area, in round numbers, for 65,000,000,000,000,000 of these tympanic membranes thus tensioned, which, divided by 16,000, the number contained in a pound, gives us a ponderable mass of 4,000,000,000,000 pounds, or *two thousand million tons* of tympanic membranes which this trifling insect, according to the wave-theory of sound, is capable of throwing into rapid vibratory motion by the mechanical operation of moving its legs! Is such a result reasonable or possible? Is it not rather an infinite impossibility, and the theory which teaches it an unmitigated imposition upon the intelligence of mankind?

It must be remembered, while contemplating this unavoidable consequence of wave-motion, that the locust is not only made capable of moving these 2,000,000,000 tons of physical matter by throwing the four cubic miles of atmosphere into undulations, but this entire mass of supposed drum-skins has to be moved from a state of rest by overcoming or annihilating its *vis inertia*, carried a certain distance, brought to rest, and again started, and so on at the rate of 440 such stops and starts a second, this being the number of air-waves sent off by the insect, according to its pitch of tone, it being the middle A of the piano or that of the second string of the violin. To say that a pretended scientific theory which teaches the possibility of such a mechanical result is an infinite fallacy, is to employ tame language in regard to it.

There can be no mistake about the foregoing calculation, and hence no way for physicists to escape the annihilating consequences to their favorite theory of sound-waves, logically deduced from it. They can not say that the sound of this species of locust is not heard throughout this area, as this is a patent fact admitted by the greatest living naturalists, including Mr. Darwin. They can not deny their own uniform teaching that the only way sound is heard at all is by the tympanic membrane being bent "once in and once out" by each separate sound-wave. They can not call in question the self-evident fact that if an ear were to be stationed at any cubic inch or quarter-inch of space throughout this area it would distinctly hear this sound. Hence, the calculation I have made is based on correct mathematical and mechanical principles; and, unless Professors Tyndall, Helmholtz, and Mayer are prepared to accept the result, and believe that an insect by the simple movement of its legs in rasping the nervures of its wings is capable of shaking *two thousand million tons of physical matter*, as heavy and as difficult to shake as that much *lead*, they must of necessity abide the only logical consequence, and abandon the wave-theory as an unspeakable scientific fallacy!

This calculation, involving the idea of shaking *two thousand million tons* by means of the physical strength of an insect incapable of stirring a single ounce weight is no doubt entirely beyond the mathematical comprehension of the reader. In fact, it is difficult to grasp the idea, so as to realize it in its true signification, of what a single *million* amounts to. To simplify the problem, I will try to bring the matter temporarily within human conception, and at the same time do away with the necessity of imagining tympanic membranes stationed in what may be supposed impossible positions, such as at every quarter-inch, so that even this apparent exaggeration shall not furnish ground for a quibble, by which to weaken the overwhelming nature of the argument.

In taking a milder view of the mathematical and mechanical consequences of the problem, we will first suppose that, according to the wave-theory, when one man hears the sound of this stridulation his two tympanic membranes, weighing but *one grain*, are actually shaken. This quantity is so trifling that these investigators, never stopping to calculate where it leads, naturally feel perfectly at ease in assuming it, or taking it for granted. I would really like to have the opportunity of asking Professor Tyndall, in an innocent kind of a way, without him suspecting what I was driving at, how much weight he supposes a common locust capable of shaking, and keeping it up for one minute, at the rate of 440 oscillations a second. I think he would not venture to suggest over one *ounce*, if that much, as this would be more than fifty times its own weight. Suppose he even put it at an ounce. Then how easy it would be to explode the wave-theory by showing him that if 8,000 men should stand together around this locust and listen to its stridulation, their 16,000 tympanic membranes, actually weighing *one pound* avoirdupois, must necessarily be bent "once in and once out" 440 times a second, if there is any truth in the wave-hypothesis! How would it be possible for this great physicist to reply?

Then, as these 8,000 men can conveniently stand on half an acre of ground, and as there are over 5,000 half-acres within the four square miles permeated by the sound of this insect, it becomes evident to a schoolboy that men enough might stand within the limits of this area, and all listen to the locust at the same time, to have their *five thousand pounds of tympanic membranes* oscillated or bent "once in and once out" 440 times a second while the stridulation continued! Thus, taking the mildest and most unexceptionable view possible, this insect, which no one could believe capable of stirring a single ounce, is actually demonstrated, according to this theory, to shake a weight of 5,000 pounds continuously for a minute! The unanswerable character of the argument is thus brought within the comprehension of all, and shown to be beyond the power of any believer in the wave-hypothesis to controvert.

What now say these learned physicists? To admit that this insect could not shake 5,000 pounds of tympanic membranes, or the fifty thousandth part as much, at one time, as they would be honestly obliged to say, would be to abandon the wave-theory. To say, in defiance of reason, that such a result is possible, and that a mere insect could accomplish a mechanical effect evidently beyond the physical strength of a powerful horse, would be to excite the contempt of the whole educated world.

I have said that this argument, based on the movement of the tympanic membrane as the effect of sound, is the most conclusive reason against the wave-theory to be drawn from the stridulation of this locust, because the drum-skin of the ear is not an intangibility, or a something which can not be seen, weighed, and handled, but is a palpable, ponderable body, having a certain actual, determinate weight, and requiring a definite and determinate amount of *vis viva*, or mechanical force, to put it into motion, as literally and truly as if each tympanum were a mass of rock or iron. Whatever vague scientific delusion, therefore, we may have indulged as regards sound causing some sort of an indefinable tremor of the atmosphere, or system of aerial undulations, at whatever distance heard,—innocently supposed to require no appreciable mechanical force,—it is all swept away by the actual oscillation of this stubborn and ugly mass of 5,000 pounds of animal fiber, which would balance the scale if tested against 5,000 pounds of granite rock! And just as certain as a locust has not the physical power to shake that quantity of *granite* by kicking against it or rasping its legs across it at the rate of 440 vibrations a second, just so certain is the whole wave-theory a shallow and transparent scientific blunder.

Although I have modified this argument and the original calculation, temporarily, by limiting the weight of tympanic membranes to the number of men who can actually stand together and listen to the stridulation, making in this way only 5,000 pounds which this insect has to shake (evidently *fifty thousand times more than it can accomplish*), yet it is clearly manifest that my first estimate was unmistakably the correct one; for, if one tympanic membrane at any single point of the atmosphere within the four cubic miles is shaken by this sound, *it is manifestly because the atmosphere at that particular point is so agitated mechanically as to cause the drum-skin to vibrate, or otherwise it could not shake;* and hence the same agitation must necessarily occur at every other point of the atmosphere where this tone is heard, which would also equally shake a tympanic membrane if it should be there present! Thus I demonstrate, beyond all controversy, that my first calculation was correct, and that this stridulation of an insect must necessarily exert a mechanical force upon the atmosphere, by the movement of its legs, of *two thousand million tons*, if there is the slightest foundation in science or in fact for the wave-theory of sound!

These are no fancy figures of the *ad captandum vulgus* type, but the logical results of mechanical and mathematical necessity, as much so as are the figures employed by the astronomer in calculating an eclipse, or by the mechanic in estimating the weight of a steam-boiler. I therefore ask, is the reader prepared to accept such an unavoidable mathematical and mechanical result as reasonable or probable? If not, then the wave-theory, which teaches, as its most vital principle, that we can only hear sound by the vibration of the tympanic membrane, falls hopelessly to the ground, and must henceforth be relegated to the limbo of exploded scientific speculations.

The quotations I gave from these highest living authorities at the commencement of this argument (page 175), in which the theory teaches that we hear sound by the tympanic membrane bending "once in and once out" as each sound-wave strikes it, and by which such oscillations are transferred to the auditory nerve, and conveyed "afterwards along the nerve to the brain, *where the vibrations are translated into sound*," can not be explained away, nor can their disastrous effects on the wave-hypothesis be weakened in the slightest degree; neither can the result, mathematically demonstrated, by which an insect is made to exert a mechanical force of 2,000,000,000 tons, be jostled or impugned by any scientific figuring in the power of physicists, without a total abnegation and renouncement of the wave-theory.

In view, therefore, of the utter impossibility of any kind of a reply being made to this argument which will give a lease of life to the wave-hypothesis, one can hardly help sympathizing with these authors who have so ruinously involved themselves and their theory in the self-stultifying citations I have made. Favored indeed may be considered that physicist who has not been tempted at some evil hour of his life to write a book on sound, and thus to hopelessly compromise his reputation for scientific sagacity by committing himself to this unfortunate and inexcusable blunder of tympanic vibration.

At this point a single word with my scientific young friend, with whom I have so often discussed these questions, who admits that the wave-theory, with its condensations, rarefactions, and generation of heat sufficient to add one sixth to the velocity of sound, is an almost infinite fallacy, but who still believes it impossible but that *some sort of motion of the air must take place whenever sound is heard!*

Now, to settle that difficulty once for all, I will say that if there is a motion of any kind among the particles of the air as the effect of sound, it must be manifestly a movement synchronous or in periodicity with the vibration of the sounding body which generates the tone, or otherwise the tone does not cause it. No one can avoid this conclusion. Professor Helmholtz teaches this in the plainest language:—

"A periodically oscillating sonorous body produces a *similar periodical motion, first in the mass of air* and then in the drum of the ear; and the *period of these vibrations must be the same as that of the vibration in the sounding body.*"—*Sensations of Tone*, p. 16.

This being so, it amounts to exactly the same thing as the wave-theory; for, as the sound of the locust could be heard throughout every quarter-inch of the four cubic miles, *if an ear were present*, it follows that every particle of air throughout this area must keep up some kind of a vibratory motion, pendulous with the source of the sound, as long as the stridulation of the insect continues; and whether this tremor be in the form of a wave, having a supposed condensation and rarefaction, with one half of it above and the other half below the normal temperature of the air, or not, it involves the same mechanical impossibility of actually displacing and overcoming the inertia of four cubic miles of air 440 times a second, as demonstrated above.

And, what is worse, the separate molecules of the atmosphere which are displaced throughout this area, having no normal pendulous swing or vibrational number of their own, or any other oscillatory motion, only as they are forced from their state of rest by directly having their inertia overcome, must evidently be moved bodily, if at all, and brought to rest 440 times a second, without the slightest aid from the periodicity of *pendulous momentum.* The normal pendulous swing of any responding body can only come into play when the motile or exciting pulses synchronize with such fixed and definite normal oscillation; or, in other words, a responding body must be suspended or tensioned to make that determinate periodic time, which, as reason must teach us, the air-particles can not and do not individually possess. Hence, their displacement, even if it be not wave-motion, with "condensations and rarefactions," involves the absolute overcoming of the inertia of the four cubic miles of atmosphere 440 times every second while the sound continues, without any pendulous assistance whatever.

But even if it were supposable that the elementary air-particles might possess a normal pendulous swing or vibrational number of their own, it is evident that there could be but *one such normal vibrational rate,* in which case they could only give pendulous assistance to one single definite pitch of tone, or that pitch which happened to be in unison with their own normal swing!

Denying wave-motion, therefore, with its "condensations and rarefactions," and its acknowledged impossible generation of heat and elasticity in the air, while yet insisting on some other kind of vibratory motion, which involves the same thing in effect, by the shaking and displacing of four cubic miles of atmosphere, the inertia of which has to be overcome and restored 440 times a second by the stridulation of the locust, does not seem to help the difficulty in the least. My young friend, let me say to you, frankly, if you must believe in some sort of an infinitely absurd hypothesis, stick to the venerable wave-theory, as you will then have the satisfaction of knowing that you are in company with the best scientific minds of all ages.

But I am not yet through with this vital feature of the wave-hypothesis, namely, the shaking of the tympanic membrane by sound, as the reader will discover before this chapter is ended. I am prepared to show that sound does not and can not, in the nature of things, cause this membrane to oscillate at all or stir in the slightest degree, and that it is a foundationless error to suppose that Nature intended us to hear sound by any such an impossible synchronous oscillation of this so-called drum-skin of the ear.

True, a membrane not in unison may be forced into an unsympathetic tremor by the incidental air-waves generated by a sounding body in close proximity to it. Even the tympanic membrane might be so coerced; *but this is not the effect of sound,* but of an incidental movement accompanying it, and can not take place at a distance, *as in the sympathetic action of unison bodies.* But physicists, as usual, make no distinction here. Professor Helmholtz, speaking of the sympathetic response of the drum-skin of the ear, says: "the period of these vibrations must be the same as that of the vibrations in the sounding body."

Now, it needs no argument to prove that if we hear sound at all by means of the synchronous oscillations of the drum-skin, as this citation clearly asserts, *that it would be only possible to hear tones of one single pitch,* or within a shade of that one pitch, since a stretched membrane, whether it be a "drum-skin" or a drum-head, can only oscillate sympathetically, by means of sound-pulses which proceed from a unison or very nearly unison instrument.

But here comes the complete overthrow of the theory; for, as the tympanic membrane practically receives and transmits to the brain, through the auditory nerve, every conceivable shade of pitch, from 30 vibrations to 5,000 vibrations in a second, and one as effectively as another, it is perfectly clear that this can not be accomplished by its synchronous and sympathetic oscillation, since, as shown, it is not possible for it to have more than one single tension, or respond sympathetically to more than one single determinate pitch of tone, or thereabout.

This manifest impossibility of the responsive oscillation of the tympanic membrane to a thousand different periodic rates of air-waves or sound-pulses, when no other conceivable membrane or musical instrument will respond to more than one fixed and determinate rate, must strike every mind, competent to reason on the subject at all or capable of drawing any rational conclusion from premises, as an acoustical demonstration that we do not and can not hear sound by means of the sympathetic oscillations of this membrane, as the wave-theory is unavoidably compelled to maintain. Is not this clearly unanswerable?

But the impossibility of tympanic vibration does not even stop here. Its infinite absurdity will now be made more manifest than ever. Professor Tyndall tells us that,—

"The *same air* is competent to accept and transmit *the vibrations of a thousand instruments at the same time.*"—*Lectures on Sound*, p. 257.

Manifestly the only way we can know that the same air is competent to "transmit the vibrations of a *thousand instruments at the same time*" is by hearing them all "at the same time"; and I presume Professor Tyndall has an auditory apparatus capable of hearing that many all at once, or he would not have made this broad and definite statement. Reducing this "thousand" somewhat, I have, myself, listened to a large orchestra, composed of fifty or sixty instruments, all sounding their respective parts at one time, while no two of them were giving out tones exactly of the same pitch and intensity. According to the wave-theory, each instrument was sending off a different system of air-waves, each system causing the same air-particles to oscillate at an independent rate of vibration, and each driving the same air-particles through an independent and different width of amplitude, according to its loudness. And all these diverse rates of wave-motion and conflicting amplitudes of the same air-particles must take place, remember, in the aural passage, not more than a quarter of an inch in diameter, and each tone be produced by a separate system of waves, if the theory has any foundation in fact.

But even this is not the culmination of the impossibility. The fifty different and independent systems of air-waves, acting each with an independent rate of wave-motion and width of swing, transmit their conflicting impulses to the small area of this membrane at the same time; and, in order to produce the impression of the fifty different tones, this membrane must at the same instant necessarily go through with fifty independent rates of vibratory motion, with fifty distinct but independent amplitudes, involving the ridiculous impossibility of the same drum-skin moving in at least half as many different directions, with half as many different velocities, and throughout half as many different and conflicting distances, at one and the same time, since it must bend "*once in and once out*" as each wave strikes it, according to the high authority of Professor Tyndall! As the intuition of a child must at once pronounce this impracticable, it follows that sound can not be heard and is not intended to be heard at all by the synchronous vibration of the tympanic membrane; for it is certain that all of these fifty tones make each a distinct individual impression on this organ, since I found no difficulty whatever in following any instrument I chose to select, or in hearing its notes separately and distinctly by a proper act of attention.

Now, as this small membrane absolutely and unmistakably received and literally transmitted to the brain all these diverse tones, and, as the unpoetical Tyndall puts it, one "thousand" separate tones at one time, is the reader prepared to admit that it did so by sympathetically and mechanically oscillating in that many different directions, at that many rates of velocity, and throughout that many different distances, at the same time, and thus to indorse the wave-theory? To accept such a physical impossibility is to wipe out all known mechanical laws and scientific principles of motion at a single sweep. Remember the words of Professor Helmholtz, already quoted:—

"It is evident that *at each point in the mass of air* [It is even more impossible, applied to the mass of the tympanic membrane itself,] *at each instant of time*, there can be *only one single degree of condensation*, and that the particles of air can be moving with *only one single determinate kind of motion*, having *only one single determinate amount of velocity*, and passing in *only one single determinate direction*."—*Sensations of Tone*, p. 40.

No wonder, then, in view of the absolute necessities of the wave-theory, and the unavoidable fact, if it be true, that a "thousand" separate systems of air-waves congregate in the aural passage at the same moment, each with an independent rate of vibration and different degree of amplitude, that Professor Tyndall should break out as he does:—

"When we try to visualize the motions of that *air*—to present to the eye of the mind the battling of the pulses direct and reverberated—*the imagination retires baffled at the attempt*."—*Lectures on Sound*, p. 257.

But I shall take occasion to revert to this argument again, before the close of the chapter.

Let us now turn for a moment and take a look at the natural and unavoidable effect of the detailed carrying out of an erroneous theory, namely, *self-contradiction*. Although Professor Helmholtz is universally regarded as one of the most profound and careful thinkers on whatever branch of physical science he touches, and one the most likely to make this theory of atmospheric sound-waves hang together if there is any intrinsic coherence in it; and although, as seen by recent quotations, he teaches, with Professor Tyndall, and in the most unmistakable terms, that sound can only be heard by the vibratory motion of the tympanic membrane caused by the synchronous dashing of air-waves against it from a sounding body, it is nevertheless a fact as gratifying as it is natural that at certain lucid moments he intuitively contradicts himself, and thus utterly overthrows the impossible hypothesis of tympanic vibration as well as that of wave-motion. This happens, however, when he is casually directing his attention to another phase of the sound-question, namely, the office filled by Corti's arches, as they are called, and the elastic microscopic appendages of the auditory nerve ramifying the labyrinth. He then apparently forgets this theoretical disturbing power of a locust's feet, capable of throwing four square miles of atmosphere into "condensations and rarefactions" with a mechanical force sufficient to "shake" at one time *two thou-*

sand million tons of drum-skins, and sensibly gives the following death-blow to the theory he has worked so long and so earnestly to establish. Mark his words:—

"In this *transference* of the vibrations of the air into the labyrinth, it is to be observed that though the particles of the air themselves *have a comparatively large amplitude of vibration*, yet their *density is so small* that they have no very great moment of inertia, and consequently when their *motion is impeded by the drum-skin of the ear* they are not capable of presenting *much resistance* to such an *impediment*, or of *exerting any sensible pressure against it*."—*Sensations of Tone*, p. 199.

How, then, in the name of science and common sense, is the stridulation of an insect to "shake" the drum-skin of the ear and cause it to oscillate, when its sound-waves are not capable of "exerting *any sensible pressure* against it"? And if it can exert no "sensible pressure" against one drum-skin, then will this lucid and authoritative writer on physical philosophy try to inform the unscientific reader how a locust can so drive off the air-waves by simply moving its feet as to set into motion 2,000,000,000 tons of such drum-skins at one time, bending *each membrane* "*once in and once out*" 440 times a second, yet at the same time without "exerting any *sensible pressure* against it"? A more pitiable and hopelessly suicidal self-stultification does not occur in the writings of any philosopher, ancient or modern. As a stand-off, therefore, to the universal teaching of physicists that the tympanic membrane vibrates in response to sound, as the means by which the sensations of tone are transferred to the auditory nerve and thence conducted to the brain, and as a final and unanswerable overthrow of the wave-theory of sound, I only need to quote these memorable words of this greatest living acoustician and sound expert:—

"*In this transference of the vibrations of the air into the labyrinth. . . . When their motion is impeded by the drum-skin of the ear they are not capable of presenting much resistance to such an impediment* ☞ *or of exerting any sensible pressure against it.*"

Had Professor Helmholtz been a convert to the corpuscular hypothesis of sound, and had he been attempting authoritatively to annihilate the wave-theory in a single sentence, and thus undo all he has ever done or said in favor of it, he could not have used language more directly to the point than the words recorded in the above citation.

Notwithstanding this authoritative assurance that air-waves driven into the auricular passage by means of sonorous vibrations may strike against the "drum-skin of the ear" without making any "sensible" impression upon it, yet by some kind of scientific hocus-pocus this author manages to effect what he calls a "transference" of these aerial "vibrations" through this tympanic membrane "into the labyrinth," thence to the auditory nerve, and through its multitudinous appendages finally to the brain, where the same "vibrations" which are stopped by this "impediment" of the "drum-skin of the ear"—exerting no "sensible pressure against it"—are translated into sound!

Can anybody help Professor Helmholtz? If not, will somebody try to tell the unscientific reader what he is driving at? Why is it that he so persistently labors through forty or fifty pages of his book trying to devise some means of effecting a "transference" of these supposed aerial undulations through this "drum-skin of the ear" to the auditory nerve, when there is not the least use in the world for any such complicated operation, or even for any vibratory motion of the air or its "transference" through the drum-skin, as he might easily know if he would exercise his great faculties for one minute in the right direction, instead of working with might and main to ignore the simplest scientific truths in order to work out this impossible problem of wave-motion, and make it appear consistent? I deny emphatically that this physicist, if he were definitely asked, could give the slightest plausible reason for such "transference," or show any necessity for this hypothetic vibratory motion being carried to the auditory nerve in order to convey to the brain the appropriate sensations of tone.

We all know, and Professor Helmholtz evidently knows, that the infinitesimal and practically imponderable atoms of *odor* actually come into contact with the sensitive membrane of the nostril, that their impression is then transferred through it to the olfactory nerve, and thence conveyed along this nerve to the brain, where it is translated into the sensation of smell, independently of any oscillation of the nose or its membranes, without the assistance of any kind of wave-motion either of the air within the nostril or outside of it, and without the "transference" of any "vibrations" whatever to this nerve! If these corpuscles of a *real substance*—acknowledged to be such by the whole scientific world—can, by simple contact with one of the sense-membranes, have their impression transferred through it to the corresponding nerve, and thus conveyed to the brain without air-waves or hypothetic odoriferous vibrations, then, prythee, thou learned physicist, why all this labored effort in transferring sonorous impressions through the sensitive membrane of the ear by means of impossible undulations and useless vibratory motions, when the beautiful hypothesis of substantial sonorous corpuscles solves the problem exactly in the same way?

If substantial radiations of fragrance, intangible to any sense save one, can propagate themselves through the atmosphere by an unknown law of conduction and diffusion, without aerial or any other kind of undulatory motion, and be thus brought into direct contact with the sensitive nasal membrane, and through it have their impression transferred to the olfactory nerve, and thus conveyed along this nerve to the brain, producing the sensation of smell, without the "transference" through such membrane of any kind of external waves or vibratory motions, can it be considered an impossible or unreasonable assumption that sound also may consist of corpuscles alike intangible to four of the senses, be propagated by somewhat similar laws of radiation and conduction, make their characteristic impression on the membrane of the ear, and finally through it be transferred to the brain by an analagous process? Let the impartial scientific student and physical investigator decide.

If there were no other argument in favor of the corpuscular hypothesis of sound and its unbounded superiority in every respect over wave-motion in solving sonorous problems, this simple analogy existing between the sensations of sound and odor ought to be sufficient to satisfy any reasonable mind, especially taken in connection with these self-annihilating efforts of physicists in maintaining the wave-theory.

The erroneous assumption that sound is conveyed through the atmosphere by means of aerial undulations, the folly of which must by this time begin to be evident to the mind of the reader, has led to all this lamentable waste of time, ink, and paper, on the part of this accomplished German investigator, whose works in other departments of science, as well as in this, give evidence of great mental activity and profundity of thought. It is a real pity, therefore, that Professor Helmholtz had not first of all brought to bear his analytical and splendid mathematical powers on the fundamental facts and principles of the wave-theory itself, and thus have shown its complete fallacy as a scientific hypothesis, which he certainly would have done had the question flatly presented itself to his mind. Had he been fortunate enough to have made this discovery, or even to have obtained an inkling of it, while writing out his *Sensations of Tone*, he would then never have been confronted with these self-stultifying facts of his theory, or have committed himself to the labor of accomplishing a "transference" to the auditory nerve of air-waves which do not exist; or, if they do exist, meet with an irresistible "impediment" in the "drum-skin of the ear," against which they are incapable "*of exerting any sensible pressure.*"

How a theory, involving, as it necessarily does, these constantly recurring self-contradictions, or such manifest mechanical impossibilities as giving to a locust the physical strength of *two thousand million horses*, could ever have found a lodgement in the intellects of such careful investigators as Professors Tyndall, Helmholtz, and Mayer, is more than I can bring myself to imagine. Yet this very mechanical miracle of an insect, by the motion of its legs, shaking *two thousand million tons* of tympanic membranes by *bending* them "*once in and once out*" 440 times a second,—infinitely more impossible, apparently, than raising the dead,—is subscribed to without the least mental reservation by the very men who laugh at the idea of any supernatural work, or of any mechanical result being effected through miraculous interposition or without an adequate physical cause; and who even do not hesitate to ironically propose a physical *praying test*, covertly to gratify their contempt for believers in the miraculous origin of the Christian religion!

This chapter, extended as it is, would be incomplete without a brief examination of the remarkable phenomena of *over-tones, resultant tones*, &c., so elaborately and critically treated in the great work of Professor Helmholtz on sound, called the *Sensations of Tone*, already so frequently referred to and quoted from during the progress of this review.

In addition to the acoustical importance of these most complex of all the problems connected with sound production and propagation, they appear to be regarded by physicists as specially illustrative of wave-motion and its effects, and as clearly explicable on no other hypothesis,—while to the casual observer, after reading the explanation of Professor Helmholtz, it would be regarded as futile in the extreme to attempt their solution on the hypothesis of corpuscular emissions, as here maintained. I therefore deem it a fitting subject, in connection with one or two collateral questions, on which to devote a few pages in bringing this long chapter to a close.

Over-tones, or "*partial tones*" as they are sometimes called, are faint secondary sounds of a higher pitch than the primary or fundamental tones which generate them, and are heard by a cultivated ear, and by a proper act of attention, accompanying the sounds of strings, pipes, reeds, &c. They are always the effect of a single primary tone.

Another class of secondary sounds are called *resultant tones*, or *differential tones*, which occur as the result of a chord, such as a *third* or a *fifth*, and are faintly heard as low, droning sounds, always deeper than the lowest note of the chord which generates them, and often as much as three or four octaves deeper than the lowest generating note. It is maintained by Helmholtz, and no doubt correctly, that the vibrational number of this resultant tone is always equal to the difference between the vibrational numbers of the two generating tones. That is, if the two notes of the chord are fifty vibrations apart, whatever portion of the audible register they may occupy,—even if one is five hundred and the other five hundred and fifty vibrations a second,—the resultant tone will have but fifty vibrations in a second, or the number constituting the difference between them. Hence, he calls them "differential tones."

This eminent investigator devotes much time and many pages of his work to the analysis and elucidation of these secondary sounds, and may almost be said to be the discoverer of them, since he is the first to classify them and point out the true mode of recognizing them, and thereby of demonstrating their actual objective existence in the air, thus meeting the common objection that they are only the effect of the imagination.

Among the various means employed and illustrated by this author for detecting these secondary sounds, and thus proving their objective existence, is an invention of his own which he calls a *resonator*, which enables the investigator to vastly augment the intensity of any particular tone he chooses to examine, while other tones not in unison with the air-chamber of the resonator will be excluded, or at least will not be augmented.

In using the resonator, it is first tuned to the exact pitch of the over-tone we may wish to isolate and hear, so that its column of air will sympathetically vibrate to that particular pitch of tone, while the absence of sympathetic vibration for any other note prevents, as just remarked, its augmentation, and thus enables the entire attention to be concentrated upon one tone at a time. By holding the focus-nozzle of the resonator to the ear, and directing its open mouth to the sounding string, the special over-tone with which it is in unison will be distinctly heard, as if it were the fundamental tone, even when the most sensitive ear would have failed to detect its presence without this augmenting device. In this manner, with a special resonator tuned for every possible theoretical over-tone, the presence or absence of any such tones may be absolutely known, and recorded.

These secondary sounds are much more numerous and distinct in connection with the tones of some instruments than others, particularly in connection with the primary tones of bowed strings. So rich are these in over-tones that this physicist, as he assures us, has detected as high as *eighteen*, generated in connection with a single fundamental tone, each over-tone of a separate pitch and different degree of intensity—*the loudness diminishing as the pitch becomes higher*; until they finally become inaudible even when the ear is aided by the best resonator. How much higher these partial sounds may extend beyond the register of audibility, it is, of course, not known, though the possibility of their almost infinite extension and corresponding diminution in intensity will be apparent when their true corpuscular origin is understood.

The principal object this investigator appeared to have in view, in thus analyzing and demonstrating the existence of these over-tones, was not only to prove the actual presence of such secondary sounds, but by means of them to account satisfactorily for the *quality* of tone, or that peculiar something which is sometimes designated as *timbre* or *clang-tint*, by which we can instantly distinguish the sound of a violin, for example, from that of a flute, or the note of a clarionet from that of a trumpet, even when the sounds are of the same pitch and of the same intensity. It is but fair to say that his reasons for the actual existence of these secondary sounds, as well as for their effect, as being the true cause of the *quality* of tone in different instruments, are unquestionably good and sufficient.

I do not, therefore, call in question or doubt the truth of the existence of these secondary tones, which, in a violin-string, correspond in pitch to its so-called harmonics, some ten in number, and which, as musicians know, are made by bowing lightly while barely touching the various nodes of the string with the finger. But while I admit the fact of their existence, and their effec's, I do not believe in the cause which this great physicist assigns for their generation, or the manner of their propagation through the air. I go even further, and deny *in toto* that the wave-theory of sound can even remotely account for their existence, or explain a single phenomenon connected with their occurrence. I now propose to examine briefly the solution offered by Professor Helmholtz, and adopted from him by all modern physicists, after which I will attempt their true solution on the corpuscular hypothesis.

He starts out with the assumption, or what he designates as a "law," that since the *rate* of vibration in the sounding instrument causes the *pitch* of tone, and the *amplitude* of vibration or width of swing causes the *strength* of tone, as universally admitted, so the *form* of the vibration, or the peculiar motion assumed by the sounding body, must cause the *quality* of tone. And as the *quality* of tone results directly from the combination of these *over-tones* with the primary tone, hence the *form* of the movement of the vibrating instrument must necessarily generate these secondary tones! And, of course, as all tones must be propagated by means of corresponding air-waves, it follows, if the current hypothesis be true, that the peculiar *form* of vibration in the violin-string, for example, which generates its ten different over-tones must necessarily be transferred to the air, which faithfully transmits the same vibrational form in ten superimposed systems of waves to the tympanic membrane, which finishes the work begun by the string by acting out the same tenfold vibrational form, and thus transfers the ten separate sounds to the auditory nerve! This concisely and truthfully gives the view of this eminent investigator, almost in his own language.

The Professor insists upon this so-called "vibrational form" of the string, and of the superimposed systems of air-waves as the proper cause of the generation and propagation of these secondary tones, which determine the quality of sound, as a necessary and even unavoidable conclusion, *since there is nothing else left to produce them* after assigning the *pitch* of tone to the rate of vibration, and the *strength* or intensity of tone to its amplitude! Hence, he argues, by excluding every other adequate cause, we logically prove that the *quality* of tone must result from the *form of vibration.*

Now, if the premises were correct—that every other assumption had been exhausted as a supposable cause for these over-tones —then his logic would be good. I deny the correctness of the premises, and will state the "law" in such a way as to involve what I hope to show to be the correct solution of this problem. It is as follows:—

As the *rate* of vibration causes the *pitch* of tone, and the *amplitude* of vibration causes the *strength* of tone, so the *product* of vibration—or the character of the sonorous corpuscles generated—causes the *quality* of tone! Consequently these over-tones must be produced by the action of the sound-corpuscles themselves. I appeal to the candid reader at the very start, and on the bare statement of the "law" as I have given it, if it does not strike the mind much more like a rational solution of these over-tones, which cause the quality of sound, than the supposition that a string actually goes through at one time with ten different rates of vibratory motion per second, which must be included in this idea of "form," each motion of a distinctly *different amplitude* or *width of swing*, to produce the different degrees of pitch and loudness, and then transmits this "vibrational form" to the tympanic membrane by means of a tenfold undulatory motion of the air carved and moulded into ten separate but superimposed systems of waves, in each one of which the same air-particles must necessarily pass through ten distinct rates of vibratory motion at one time! This must necessarily be the case, because, in each separate wave, Professor Tyndall assures us, the particles of air constituting it make a "*small excursion to and fro*," which is called "*the amplitude of vibration*," and therefore ten sounds, with ten separate systems of waves passing through the same atmosphere at the same time, however superimposed, must cause the same air-particles to make ten different excursions "to and fro," each excursion of an independent rate per second, and each excursion driving the same air-particles through a *different distance* or *width of amplitude*, since the ten sounds are all of different pitch and of different intensity! I ask if this correct but condensed view of the wave-hypothesis is not more difficult to believe, as the true cause of these ten different over-tones passing off from the same string at the same time, than to suppose, as I have assumed, that the substantial sonorous pulses contain within their corpuscles the intrinsic elements which constitute these tones of different pitch and intensity? However it may strike the reader at present, I venture to assure him that it will seem far the more rational view before he has finished this chapter.

The foregoing presentation of the impossible motions of the air involved in ten separate systems of waves necessary for the propagation of ten separate tones through the same atmosphere at the same time, is no exaggeration of the real difficulty which lies in the way of Professor Helmholtz and his attempted solution of over-tones by means of ten so-called superimposed systems of air-waves.

I have already shown, by an abundance of citations, that there is no possible way for the sound of a string, however complex, to be heard, according to the wave-theory, but for the tympanic membrane to take on a vibratory motion corresponding to the "vibrational form" and "number" of the string in producing such tone; and no way for the tympanic membrane to be thrown into this complex vibration but by the dashing of an equally complex combination of air-waves against it. Thus, the string must first of all assume the ten separate vibrational movements at one time to make these ten tones; then send them through the air in ten separate but superimposed and conglomerated systems of air-waves, having each a separate vibrational rate and width of amplitude, though combined somehow into one system; and finally, as they strike the drum-skin of the ear, that membrane must literally reproduce this vibrational form by taking on ten separate systems of vibratory motion, having ten vibrational numbers or rates of oscillation per second, and ten antagonistic amplitudes or widths of swing at the same time! Is such an infinitely inconceivable physical and mechanical operation as I have here described possible or even supposable? And, in view of its utter impracticability, even disguised under so-called "superposition," is not almost any other hypothesis, which pretends to offer a solution of the problem, comparatively safe? At all events, whether or not any other explanation shall be made entirely satisfactory, air-waves and tympanic vibration have already been shown in various ways to be unreasonable and impossible in the very nature of things.

But we are constantly met in the writings of Professors Helmholtz and Tyndall with what they call, as already hinted, the "superposition" of a number of systems of waves, thus blending them into one system, embracing, as they express it, the "algebraical sum" of all the different aerial motions! Now, all this sort of language only serves to cover up the difficulty without affording the least explanation. When asked to tell how such a thing is possible, they explain it in their usual lucid manner by saying that the air-particles act "according to the law of the *parallelogram of forces.*" These mysterious phrases constitute their stock in trade on this subject, and answer for a universal solution. If they stumble upon the undeniable fact that a score of distinct tones of different pitch and of different intensity can enter the aural passage undistorted, and be heard separately at the same time; and if the query propounds itself how twenty different systems of air-waves can all clash in this narrow aperture, no larger than a quill, and yet remain undistorted, and each separate tone be heard as if it alone was present, these learned physicists appear to fold their arms, shut their eyes, and reiterate "superposition," "algebraical sum," "parallelogram of forces," and expect the reader to be satisfied!

All their reference, for aid and comfort, to water-waves, with small systems of undulations crawling over the surfaces of large billows, which they constantly resort to, amounts to nothing in this case, as they will see to their astonishment at the close of the next chapter. Waves of sound do not act on the *surface of the atmosphere at all*, and can not be made to do so unless we can construct some kind of a Jacob's ladder to reach forty-five miles high.

Both these writers tell us, in a score of places, that sound-waves can only consist of "condensations and rarefactions of the air," each tone having a degree of condensation corresponding to the width of its amplitude (loudness) or rate of oscillation "to and fro" (pitch). Hence, such a thing as *crest* or *sinus* is out of the question in so-called air-waves; and therefore the superposition of small crests upon the surfaces of large ones, to which reference is made in water-waves, forms no manner of illustration of the intermingling of air-particles in these so-called "condensations and rarefactions."

Of course, the common-sense reader would say, if we can hear twenty distinct sounds at one time, which we certainly can, and which is proved by the fact that we can isolate any particular tone out of that number to which we direct special attention, then it must follow that within this narrow aperture of the ear there are twenty different degrees of condensation of the same air-particles at the same time, or else that many sounds could not co-exist in the aural passage on the principle of air-waves. Would not this be the only sensible and logical conclusion? Professor Helmholtz emphatically admits that such multiple *condensation* of the same air-particles at the same time is impossible:—

"Two different *degrees of density*, produced by two different systems of waves, *can not co-exist in the same place at the same time*."—"It is evident at *each point in the mass of air at each instant* *ne, there can be only one single degree of con-* *ιtion.*"—*Sensations of Tone* pp. 40, 42.

:ence, inevitably it follows, if a sound- e is constituted of a distinct *condensa-* and a *rarefaction*, that but one sound exist in the aural passage at one time; there can be no "superposition" of *'ensations* or of the mere *squeezing of the ɓarticles together*, whatever "algebraical " or "parallelogram of forces" may be ıght to bear on the proper crests and ses of water-waves. Think of twenty nct tones from as many different or- ;tral instruments, all occupying one ll column of air an inch long and the of a straw, that each sound is consti- d alone of such a "condensation and faction," and that these twenty differ- legrees of *density* and as many different ·ees of *rarity* are all acting at one in- t on this same trifling mass of air, thus ing twenty separate impressions on the tory nerve! Can any intelligent mind pt the idea that this conglomerate :ure of *density* and *rarity*, and it alone, ıg on these air-particles, is sufficient ccount for twenty defined and dis- :ly audible musical sounds?

ı the whole of Professor Helmholtz's ; on sound, it is a fact that he makes one single weak attempt to explain t he means by this "superposition" of systems of air-waves, or what we are nderstand by this "algebraical sum" e aerial motions constituting a number ıch separate systems. His attempted anation is apparently so cautiously poken and so rich in scientific poverty I can not help quoting it. Yes, I will e the whole of it, constituting all there ' say about this "algebraical sum" of lifferent motions acting on a separate ·ticle of air," to which I ask the reader's ıtion:—

"The displacements of the particles of air are compounded in a similar manner [to water-waves]. If the displacements of two different systems of waves *are not in the same direction they are compounded diagonally;* for example, *if one system would drive a particle of air upwards, and another to the right, its real path will be obliquely upwards towards the right. For our present purpose there is no occasion to enter more particularly into such compositions of motion in different directions.*"—*Sensations of Tone*, p. 42.

Here the reader has all there is to be said in elucidation of this fundamental principle of the wave-theory, which necessarily requires the same "particle of air" situated in the aural passage to embody in itself the "algebraical sum" of all the motions of twenty distinct systems of waves sent off from an orchestra of that many instruments, each system having a different width of swing and different number of oscillations per second,—one system driving the particle of air upward, another perchance downward,—one sending it to the left, another to the right,—one hitting it "obliquely," another "diagonally,"—the whole twenty systems making it the battledore and shuttlecock of this contradictory hypothesis, which, after it has been acted on by all these systems at one time and in twenty different directions, with that many different velocities and throughout that many different distances, is still capable of transmitting the result to the auditory nerve in twenty distinct and symmetrically formed musical sounds, as the "algebraical sum" or "superposition" of all these contradictory motions! No wonder the "parallelogram of forces" has to be called in to aid such a muddle as this. Yet this is "science"!

I do not intend that the reader shall overlook what might be strictly called a scientific *dodge* resorted to by Professor Helmholtz in the last quotation. After elaborately showing how two systems of water-waves can collide and be superimposed by the crests of one system being added to those of another, he instantly shifts the solution when he comes to treat of sound from the *waves* to the *particles* constituting them. He does not say a word about the *particles* constituting water-waves, or their "real path" under the action of two forces, since their motion is entirely a different thing from that of the onward moving swell constituting the wave proper, to which he gave his whole attention. He dwells lengthily on the superposition of little water-crests compounded with larger crests, *without reference to the motion of the particles of water constituting them*, but the moment he comes to apply the analogy to sound he drops the combined movement of the air-waves and goes to work to show how a single "*particle of air*" may be driven "upward" by one system of waves, and "to the right" by another, which two forces compounded or "superimposed" will send this particle "obliquely"! Why this sudden shifting from the motions of *water-waves* and their "superposition" to the motions of *particles of air* constituting sound-waves? *Evidently because no such thing as air-waves has an existence in any true sense, as compared to water-waves or any other proper wave-motion.* True science does not require temporizing dodges or shifts of any kind.

But look again at this singular passage last quoted. Instead of telling us, as he does, that "if one system would drive a *particle of air upwards* and *another to the right, its real path will be obliquely upwards towards the right*," why does he not try to tell us what would be "its real path" if one wave should strike it and drive it upward, and another should strike it at the same time and drive it downward,—if one wave should send it to the right and another to the left,—if one should hurl it "obliquely" and another at the same instant should hit it with equal force and drift it "diagonally" in an opposite direction,—and if the twenty systems of waves should all act on the same principle, each manipulating the same "particle of air" in the aural passage, and all combining to send it in ten opposite directions at the same time? He prudently avoids any such self-stultifying inquiry as this, and wisely concludes—"*For our present purpose there is no occasion to enter more particularly into such compositions of motion in different directions.*" This is a specimen of so-called modern science, which claims to grapple fearlessly with the most abstruse and difficult problems!

The truth is, the particles of air in the aural passage, when twenty diverse systems of sound-waves are entering the ear at the same time, if there is any truth in the wave-theory, are just as liable to be hit and driven in ten directions diametrically opposed to ten other impulses, and thus to stand perfectly still under their equally compounded blows, as to move at all in any direction or to any extent! What, then, becomes of the twenty tones? They are all silenced, of course, as they can only be heard by the periodic oscillations of the air-particles in their "excursion to and fro" constituting their respective systems of waves. But since there would be no motion of the air-particles under the counteraction of ten equal forces in opposite directions, the twenty tones, as any one must see, would necessarily cease. Is it possible that our hearing of twenty different sounds from an orchestra of that many pieces depends upon any such acoustical contingencies as this accidental commingling of waves here pointed out? Yet even this possible neutralization of aerial motion, under counteracting impulses, is also included in such meaningless verbiage as "superposition" and "algebraical sum."

In view of all these contradictory results of wave-motion, is not the corpuscular assumption, that the twenty distinct sounds of different pitch and different intensity enter the ear by means of twenty corresponding systems of substantial sonorous pulses, infinitely more consistent, beautiful, and every way reasonable? That it is so will even yet be made entirely clear before this chapter is finished.

To show that I do not deal in guess-work when speaking of *ten* partial or over-tones heard in connection with the primary tone of a violin-string, each of a different pitch and of a different degree of intensity or loudness, I will give the exact words of Professor Helmholtz:—

"When a string is excited by a violin-bow, and speaks well, *all the upper partial tones* which can be formed by a string of its rigidity *are present, and their intensity diminishes as their pitch increases.* [That is, they grow weaker as they get higher.] . . . The upper partials in the compound tone of a violin are heard easily, and will be found to be strong in sound if they have been first produced as so-called harmonics on the string by bowing lightly while gently touching a node of the required partial tone. The strings of a violin will allow the harmonics to be produced as high as the sixth partial tone with ease, and with some difficulty *even up to the tenth.*"—*Sensations of Tone*, p. 133.

I have not, therefore, misconceived nor misrepresented the explanation of over-tones as given by this authority. As each one of the ten harmonics of a violin-string is produced by touching the proper node, and thus physically and mechanically throwing the string or a particular section of it into a corresponding *rate* and *amplitude* of vibration, it follows, if the solution of Professor Helmholtz is correct, that these ten harmonic over-tones are actually produced in connection with the primary tone in the same manner, by eleven (including the primary) systems of vibratory motion of the string and its various sections progressing at the same instant, each of different amplitude and at a different rate of oscillation per second! And, as before observed, since no sound can be heard without a corresponding system of air-waves and a corresponding system of tympanic oscillations, there is no possible escape from the conclusion that the same string, the same air-particles, and the same tympanic membrane, must be capable of eleven different and antagonistic amplitudes and rates of oscillation at the same instant! I again ask, is such a thing as this possible? To show that it is not, Professor Helmholtz, as already quoted, unmistakably gives his testimony as follows:—

"*Any particle of air can, of course, execute only one motion at one time.*"—"It is evident that at each point in the mass of air, at each instant of time, there can be *only one single degree of condensation*, and that the particles of air can be moving with *only one single determinate kind of motion*, having *only one single determinate amount of velocity*, and passing *only in one single determinate direction.*"—*Sensations of Tone*, pp. 40, 222.

How, then, in the name of reason and science, can the same air-particles receive and transport eleven different superimposed systems of undulations, each system causing these air-particles to move at a different number of swings per second, at a different velocity, and through a different distance, at one and the same instant? Really, opposing the wave-theory as I am now doing, I have no language at my command in which to so effectually declare the utter impracticability of the hypothesis as is made use of in the above sweeping generalization by Professor Helmholtz.

Professor Tyndall is equally explicit on this subject, admitting tacitly and unmistakably in a single sentence that sound does not and can not pass through the atmosphere by means of air-waves. I ask the reader's special attention to the language of this eminent authority:—

"I have already had occasion to state to you that when *several sounds traverse the same air, each particular sound passes through the air as if it alone were present.*"—*Lectures on Sound*, p. 281.

A more point-blank contradiction of his teaching in numerous other passages could not be put into language, as will be prominently pointed out in the next chapter. It is enough to say here that this statement shows conclusively, though unintended, that eleven sounds passing through the same air at the same time, "*each particular sound . . . as if it alone were present,*" can not be accomplished by eleven systems of air-waves, since it is well known that such air-waves, the same as that many systems of water-waves, must conflict and naturally interfere with each other, mutually destroying or neutralizing each other whenever the crests of one system happen to fall into the troughs of another, as eleven different systems would be necessarily and continually doing, as Professor Tyndall well knows, and teaches in a score of places. Hence, the above quotation alone overthrows the hypothesis of these eleven different over-tones being constituted of eleven systems of superimposed air-waves, if there was not another consideration to be urged against it.

But this impossible occurrence of eleven conflicting systems of vibrational movements in a single string, and of eleven antagonistic systems of air-waves sent off from the same string at one instant, each system of a different amplitude and having a distinct and independent number of oscillations of the air-particles per second, does not constitute the whole nor the worst of this impracticable theory of over-tones invented by Professor Helmholtz, and copied by Professors Tyndall and Mayer. As I have already intimated, these writers do not rest satisfied till they have carried these eleven antagonistic rates of vibratory motion and widths of swing to the tympanic membrane, since they distinctly tell us that these oscillations are exactly reproduced from the eleven systems of air-waves on this *drum-skin of the ear*, which takes up and literally acts out all these conflicting and contradictory motions at one and the same time,—which necessarily involves the mechanical impossibility of a bit of membrane, about a third of an inch in diameter, stretched across the auricular passage, keeping up eleven distinct systems of superimposed vibrational movements, each system of a different rate per second and each having a different and independent amplitude or distance of motion!

By turning back to the important quotations already made from their works (pp. 175, 176), it will be seen that these writers distinctly assume what I have here stated, namely, that this diminutive membrane of the ear not only acts out the eleven vibrational numbers represented by the tones of the violin-string, oscillating with as many different amplitudes and vibrational rates per second, but they even teach, as quoted from Professor Tyndall, that a "thousand" complex and conflicting systems of air-waves have their vibratory motions reproduced on this delicate drum-skin of the ear!

In view of the paramount importance of the subject, I shall be obliged, therefore, prior to further investigating the cause of over-tones, resultant tones, &c., to digress sufficiently to again present and meet this vital question of tympanic vibration in its new and various phases, as presented by Professor Helmholtz in his able and exhaustive work on the office filled by the different parts of the ear; and shall undertake to show that physicists are wholly mistaken in this fundamental principle of the wave-theory, and hence are mistaken in the whole theory, since it is, in fact, upon this the entire superstructure rests. As this learned investigator deems the vibratory motion of the different parts of the ear in response to tone as the only means of hearing so essential to the current theory of sound that he devotes *forty* pages of his book to that special question, the reader will surely pardon half a dozen pages in reply.

In this general denial that sound is heard or intended to be heard by means of the vibratory motion of the tympanic membrane in response to whatever pitch of tone, I wish here to guard against what might appear to be a conflict with observed facts. I do not claim that this "drum-skin of the ear," rigid and circumscribed in area as it is, could not be jarred into slight tremor, apparently, by a very loud sound in close proximity, such as that of a powerful steam-whistle,—though really not by the sound at all, when we come to look at the matter critically, but by the tremor of the air *thrown into agitation by the same vibratory motion which generates the sound.* Such a tremor of the air near the whistle might even jar the fingers, or lips, or nose, as well as the whole ear. But it is a superficial view to suppose it to be the sound which effects this result, because the sound occurs simultaneously and is generated really by the same vibratory motion which incidentally shakes the air for a limited distance around. This distinction I have already made in several places in the preceding argument. As an example, the reader no doubt recollects the exposure of Professor Tyndall's memorable fiasco on magazine explosions and the effects of their "sound-waves" in breaking windows! (See page 103 and onward.)

Sound, proper, can only shake such bodies as are themselves capable of making a musical tone, and whose tension at the time allows them to oscillate normally, if started, with the same or nearly the same vibrational number; or, in other words, with the same or nearly the same number of swings per second that the sounding body makes which produces the exciting tone. The reader, I trust, can understand this.

I therefore claim that if the tympanic membrane, the ear, the nose, the lips, or the fingers, should jar or tremble as the apparent result of a loud sound, it is but the incidental effect of the vibration which generates the tone, the same as the air-waves themselves sent off by this sounding body for a limited distance around are but the incidental effect of such agitation, and not a part of sound-propagation, as already shown in several places. So far from such incidental shaking of the tympanic membrane, if it really occurs, being the means by which we hear sound, as all writers on the subject take for granted, it would rather be a hindrance to our analyzing or appreciating the tone properly, if so powerful as to actually jar this organ, just as an intensely bright object presented to the eye would so agitate and distract the retina as to prevent the accurate examination of its outline.

In opposition to this view, it is claimed by Professor Helmholtz that the tympanic membrane has been distinctly felt to vibrate to sonorous pulses, and that beats from two organ-pipes slightly out of unison have been reproduced by attaching a delicate style to the auditory bone (the *columella*) of the common duck, the style being observed sensibly to vibrate as the beats struck the drum-skin of the duck's ear! Here, again, I am compelled to charge these writers with the most inexcusable superficiality in mistaking the reactive effect of the tone, through the nerves of sensation, for the direct mechanical effect of the sound upon this columella of the duck. To show the shallowness of this reasoning, let the duck be killed, without marring or deranging in the slightest degree the auditory apparatus, leaving the style connected as before with the columella, and then bring to bear the organ-pipes, with their "beats," and if the drum-skin, the auditory bone, and the style respond as when the duck was alive, I'll give up the argument! The explanation of all such effects, as just hinted, lies in the simple and natural reactive result of sound which first produces the sensation on the brain through the sensitive tympanic membrane and auditory nerve, and then reacts in throbs corresponding to the beats of the organ-pipes on the auditory bone, and no doubt to some extent on all other parts of the duck's body!

These great physicists ought to know that they can construct artificially a tympanic membrane, even more delicate and of much finer material than that constituting the drum-skin of the duck's ear. Yet they never think of testing such a membrane, and of that size and rigidity, connected in the same manner with an artificial columella, using their beating organ-pipes and sensitive style; but reason like children, that because they see such effects produced in a *live duck*, having a reactive nervous system, it must necessarily be the gross mechanical effect of objective air-waves dashed against the drum-skin, instead of the subjective reaction of sense-shocks communicated from the brain through the nerves back upon these auditory organs!

This case of the duck and the vibrating style is similar to that recorded of the *mysis* or the *opossum-shrimp*, whose so-called auditory hairs were experimented on by V. Hensen, as related by Helmholtz in his *Sensations of Tone*, p. 225. Hensen found, on sounding a keyed horn, that certain hairs of this crustacean would quiver in response to tones of a determinate pitch, while other hairs would vibrate to other tones. Hence, the profound (!) scientific inference that these hairs, without the least regard to size or length, were *tuned in unison* to certain pitches of tone, and vibrated sympathetically as such notes were struck on the horn!

One would have thought that such careful investigators would have been struck with the acoustical anomaly of hairs vibrating to certain tones without corresponding difference in size, length, or tension, and would have been led to inquire why this result was never witnessed in the sympathetic vibration of strings, rods, or any other kinds of musical device. A tyro in the investigation of acoustical phenomena would have made this his first inquiry, and have stopped right there till the mystery was solved.

But neither Hensen nor Helmholtz appeared to be capable of noticing this bottom fact, or of looking below the surface idea of the mere motion of the hairs as certain pitches of tone occurred, and thus grasping the beautiful thought that these tones, after reaching the ganglionic center of this animal, and being there translated into sounds of different pitch, reacted through its nervous system upon these auditory hairs, whose roots connected with these nerves,—certain nerves conducting tones of one pitch, while other nerves leading to other auditory hairs, without any regard to their length or size, conducting tones of a different pitch! The possibility of such a thing as reactive effect through the sense-nerves being produced, and thereby causing certain parts or organs to quiver, never entered the minds of these learned investigators. They superficially observed certain auditory hairs of this shrimp to vibrate as certain sounds were produced on the horn, and at once jumped to the conclusion, like children, that these hairs must be tuned in unison with that particular tone, and therefore vibrated as the effect of that particular system of sonorous waves dashing against it.

But if Helmholtz and Hensen wish to satisfy themselves of their mistake, and to become convinced that these results can only be explained, as here suggested, by the reactive effects of these tones through the nervous system of the shrimp, let them first kill this animal, as suggested in the case of the duck, and they may then blow their horn till the crack of doom, and they will find, to their individual improvement, that, so far from these auditory hairs being tuned in unison, they will utterly fail to respond, demonstrating that their tremor was the effect of subjective reaction, and that they did not move as the objective result of hypothetic sound-waves.

In like manner, if any part of our own ear is felt to vibrate by sounds of a certain pitch, we may be sure that it is subjective, as the reactive effect of the tone through the sense-nerves leading from the brain to the affected part, and not the objective result of external air-waves which have no existence in the propagation of sound except in the superficial imagination of physicists.

Analogous to this view of reaction in sound, it is well known that powerfully pungent odor, when it has produced upon the brain the sensation of smell, acting through the sensitive membrane of the nose and the olfactory nerve, may so react through the nervous system as to not only cause a shiver in certain parts and organs and force water out of the eyes, but may easily produce a reactive shock which will cause the whole physical organism to shudder! Yet what physiologist or physicist would be so superficially innocent of all logic and reason as to conclude that it was the mechanical and objective force of the imponderable granules of odor striking against the membrane of the nose which jarred the whole body and condensed the fluids of the system into tears? How simply and beautifully could the vibratory sensation felt in the tympanic membrane be accounted for if physicists would reason about sound and its direct and reactive effects in the same manner as they would be compelled to reason about the action of the somewhat analogous corpuscles of odor! As well might they descant learnedly about the nasal membrane and the organs of olfaction being thrown into vibratory motion by fragrant pulses or odoriferous waves issuing from a lump of ammonia, ignoring the substantial corpuscles of this perfume, as to continually harp upon the same kind of philosophical nonsense about sound and the effects of the superposition of supposititious air-waves upon the drum-skin of the ear!

It has already been shown, a few pages back, by the most demonstrative mechanical and mathematical argument within human imagination, that the tympanic membrane can not vibrate in response to sound, since if it did so oscillate or was so intended to oscillate as the natural mode of hearing tone, it necessarily involves the shaking of *two thousand million tons* of such ponderable matter by the stridulation of an insect not capable of stirring an ounce by exerting all its strength. No physicist can reply to that argument against tympanic vibration, and I will venture to say that no one will ever attempt it, notwithstanding it saps the very foundation of the wave-theory, as the most superficial reader must see.

But even if it were conceded that this membrane can actually vibrate sympathetically as the mode of hearing sound, or as the means by which sonorous impressions are conveyed to the auditory nerve, still, as I have already shown, this would absolutely limit us to the hearing of one single pitch of tone *distinctly*, while we might hear *faintly* the slight variation from this vibrational number,—not to exceed a semitone either way from absolute *unison*. I recently promised to revert to this important matter, so vitally important to the wave-theory if true, but if false so fatally destructive to the reasoning of physicists on the structure of the ear, and the true mode of hearing tone; for, if tympanic vibration breaks down, there is not an unbiassed physicist living who would not be compelled to renounce the wave-theory of sound, since of what use would be air-waves in the propagation of sound if the tympanic membrane can not respond to them?

As already intimated, and as is well known even to the unscientific, a string, tuning-fork, reed, pipe, or membrane, however tuned, will not be thrown into appreciable vibratory motion in sympathetic response to the tone of another instrument unless it is tuned in unison or very nearly in unison with such exciting tone; or, in other words, unless its own vibrational tension and number correspond to the number of periodic pulses generated by such actuating instrument. Hence, if the tympanic membrane were intended to vibrate sympathetically at all as the mode of conveying sound to the auditory nerve, as physicists are necessarily obliged to claim, it could not sensibly stir, as observation proves, unless its own vibrational number, or normal tendency to oscillate when put into motion, corresponded to the vibrational periodicity of the exciting tone. A sounding instrument, such as fork or string, tuned to any other pitch save that of unison with the vibrational number of this membrane, or very near it, could not, of course, stir the drum-skin of the ear; and hence, if there is any truth in the wave-theory, such a tone would not be heard at all, since this vibratory motion of the drum-skin is the only mode of hearing sound! Can any inductive mode of reasoning on any question of science be more conclusively certain than this?

It is true that Professor Helmholtz partly foresees this difficulty, and to this extent tries to guard against it; but he evidently does not fully realize its fatal consequences to the wave-hypothesis, as I will clearly show. The infinite impossibility of this diminutive membrane, but a third of an inch in diameter, vibrating in sympathetic synchronism with tones of all possible vibrational numbers or degrees of pitch seemed to flash momentarily across his thoughts, like the vision of some miracle of which, though we might wish an explanation, we must content ourselves to remain in the dark. He goes so far, however, in trying to partially provide for it, as to tell the reader that an instrument like a membrane which comes quickly to rest after being thrown into vibration does not require such accurate unison in the exciting tone as would a tuning-fork, which, when once excited, vibrates a long time! This is true enough but still, how little does it help this terrible difficulty! For, while the fork, owing to this enduring oscillation when started, requires the most exact unison to sympathetically excite it, the membrane requires very nearly unison, or not to exceed the variation of a semitone either way, as he is himself forced to admit in the most explicit language, when speaking of the "parts of the ear," as follows:—

"The *intensity* of *sympathetic vibration* with a *semitone difference of pitch* is only *one tenth of what it is for a complete unison*. . . . Hence, when we hereafter speak of individual *parts of the ear vibrating sympathetically with a determinate tone*, we mean that they are set into *strongest motion by that tone* [unison], but are also set into vibration *less strongly* by tones of *nearly the same pitch*, ☞ *and that this sympathetic vibration is still sensible for the interval of a semitone*."—*Sensations of Tone*, p. 216.

Frankly and unmistakably, then, let it be understood, this highest living authority on sound admits that "parts of the ear" can not *sensibly vibrate* by sympathy more than a "semitone" out of unison with any "determinate tone"! How, then, in the name of acoustics, is the "drum-skin of the ear" to sympathetically vibrate to any "determinate tone" when it is out of unison with the vibrational number of this membrane more than the "interval of a semitone"? He clearly admits such sympathetic vibration impossible, unless within this circumscribed limit; and hence, if the wave-theory be true, that the tympanic membrane is intended to sympathetically vibrate at all in response to sound as the mode of transmitting tone to the auditory nerve, as all authorities tell us, then let it be proclaimed to the scientific world that this leading sound expert and investigator has shown that it is impossible for the human ear to recognize any tone or hear any sound save that of one determinate pitch, with a faint but rapidly diminishing margin of a "semitone" either way from the proper vibrational number of the tympanic membrane!

Is it possible to believe that this universally accepted scientific theory, expounded by its ablest advocates, first teaches that the tympanic membrane, one of the principal parts of the ear, vibrates in response to all audible sounds of the musical scale, including every degree of pitch, bending "*once in and once out*" as each sound-wave strikes it as the only means of hearing tone, and then that the same theory in the hands of the same highest living authorities turns right round and teaches exactly the opposite, as just quoted, namely, that the "*individual parts of the ear*" which respond by "sympathetic vibration" can only vibrate to a sound when within "the interval of a semitone" of "*complete unison*"? The world is challenged to find any theory in the annals of scientific investigation, ancient or modern, not excepting the Ptolemaic system of astronomy, containing as many point-blank and self-stultifying contradictions as have been pointed out in this wave-theory of sound during the preceding argument. Yet the exposure of its multitudinous absurdities and self-contradictions has hardly commenced. I ask the intelligent reader, in view of the above, if it is possible for the wave-theory to remain unshattered as science while receiving such staggering blows?

But I have evidence from this same authority even more definite than this, overthrowing tympanic vibration as Nature's plan of transmitting tone to the auditory nerve. When discussing another phase of the sound-theory he naturally forgets the absolute necessity of this membrane of the ear vibrating sympathetically to tones of every degree of pitch throughout the musical scale, and deliberately teaches that a stretched membrane will respond only to a tone which happens to be in "*unison*" with it, thus confirming my argument that the drum-skin of the ear is necessarily confined to one pitch of tone if it vibrates at all.

Thus, when instructing the reader how to detect *combinational* or *resultant tones*, which, as already intimated, are low secondary sounds generated by the two tones of a chord, he shows that a stretched membrane tuned in unison with such resultant tone will instantly be thrown into sympathetic vibration *whenever the two notes of the chord are sounded*, thus proving the presence of this resultant tone in the air, even though it may be so feeble as not to be distinctly audible, and thus demonstrating that these resultant tones are not the effect of the imagination, as some have supposed,—while he goes further, and assures us that this membrane, thus tuned in unison with such resultant tone, *will not stir when either of the two generating tones of the chord is sounded separately*, simply because neither of such primary tones is in "unison" with it! Speaking of these combinational tones, his words are:—

"Their objective existence in the mass of air *can be proved by vibrating membranes tuned to be in unison with the combinational tones. Such membranes are set in sympathetic vibration immediately upon both generating tones being sounded simultaneously*, but *remain at rest if only one or other of them is sounded.*"—*Sensations of Tone*, p. 235.

Here, then, he himself admits that stretched "*membranes*" will not vibrate sympathetically except in response to "*unison*" tones! How, then, is the tympanic membrane to vibrate to any except one single pitch of tone, and that tone the "unison" to its own vibrational number?

I could extend the annihilating self-contradictions of this eminent authority *ad libitum*, showing that whenever he is not treating directly on the tympanic membrane or some other part of the ear, and the absolute necessity of it vibrating in sympathy to all degrees of pitch, he invariably takes the common-sense view of the matter, and the view which even a schoolboy knows to be the correct one, namely, that no instrument can be thrown into sympathetic vibration by the tone of another unless the two are in unison or very near it. Take one other example, where he is speaking of a singer having the power of throwing a piano-string into sympathetic vibration by directing the voice against it. His words are:—

"The more *exactly the singer hits the pitch of the string, the more strongly it vibrates.* A *very little deviation from the exact pitch fails in exciting sympathetic vibration.*"—*Sensations of Tone*, p. 61.

How sensible this great physicist can be when he confines himself to scientific facts, and is guided by the unfailing laws of acoustics? But how absurdly childish he becomes the moment he branches off into the self-contradictory superficialities of the wave-theory! Can any one imagine a more abrupt transition from sound reason to insipid nonsense, than, after reading the above, to turn back to pages 175 and 176 and read what this same author and Professor Tyndall say about the tympanic membrane vibrating sympathetically to tones of every degree of pitch, bending "once in and once out" as each sound-wave strikes it, from the lowest note of the church-organ to the highest tone of the piccolo-flute?

The fact is, the tympanic membrane, if it vibrates at all in sympathetic response to tone, must act as all other membranes act, and that is, respond to only one determinate pitch—its own vibrational number; and Professor Helmholtz knows it whenever he steps outside of the wave-theory, and is thus momentarily freed from the spell of its blinding influence. But this absurd philosophy having taught him from his youth up that we can only hear sound by the vibratory motion of the tympanic membrane, he has not even in his ripe manhood the power to stamp down, crush out, and break away from an erroneous hypothesis which contradicts his very senses and upsets the foundation-laws of acoustics and mechanics, but goes on advocating what he must know, unless mentally blinded, to be infinitely impossible in the nature of things.

Now, as everybody knows that a stretched membrane can only respond to one determinate pitch of tone, or, at most, can not vary from it even faintly more than a semitone either way, and as we all know that we hear tones of every degree of pitch throughout the musical scale, and all the separate degrees with equal facility, it becomes clearly demonstrative, as must be evident to the commonest intelligence of the unscientific reader, that the hearing of sound is independent of any vibratory motion whatever of this membrane. Is not this as acoustically certain as that we hear sound at all? Hence, the wave-hypothesis, depending as it does on tympanic vibration for its existence, necessarily and absolutely breaks down.

I therefore repeat my deliberate conviction, which I believe the judgment of the scientific world, upon re-investigation, will indorse, that this assumption of tympanic vibration as the means by which the sensations of tone are transmitted to the auditory nerve, lying as it does at the foundation of the wave-theory, is an error of so grave and glaring a character that its exposure must lead to the immediate revolution of the current hypothesis of sound; and that if physicists, who have already committed themselves by writing elaborate works on the subject, shall feel indisposed to undo what they have accomplished with so much labor and effort, the work must be relegated to other investigators in time, equally competent, who will arise and take up the imperfect chain of argument introduced in this monograph, and carry it out to a systematized analysis of the whole question.

I only regret that the discussion has unavoidably forced me into such direct antagonism to Professor Helmholtz, and compelled me, though reluctantly, to expose his utterly inexcusable contradictions and mistakes in his efforts to harmonize what is intrinsically incongruous, for otherwise I might have looked upon his great analytical ability to aid the new hypothesis, and thus assist in revolutionizing the theory of sound as no living physicist, perhaps, would have been so capable of doing, had the matter been brought to his attention under less embittering circumstances.

But this vital doctrine of tympanic vibration has become too important a question, and the very life of the wave-theory of sound is too intimately involved in the truth or falsity of this single proposition, to allow the discussion of it to drop quite yet. I propose to show still further the inexplicable involvement of Professor Helmholtz in his almost insane efforts to harmonize so utterly false a theory as that of wave-motion with so fundamentally erroneous and self-contradictory a principle as tympanic vibration.

He announces an important law, which turns out to be as amusing as it is absurd. He admits, in the first place, as he is necessarily compelled to do, that the tympanic membrane, like all other membranes, has a normal "vibrational number" or periodic swing of its own, corresponding to its size, weight, and tension, of which the most ordinary student of science is well aware; and while acknowledging, as just quoted, that a membrane can only vibrate sympathetically to a tone which happens to be in "unison" to its own normal rate of oscillation, or, at farthest, within a semitone of unison, yet he seems wildly to insist, in his apparent confusion, that this membrane of the ear, unlike any other membrane, and without pretending to any special reason for it differing from other membranes in this regard, will vibrate in response to *every audible pitch of tone, whether in unison or not*, simply because the wave-theory requires it so to vibrate, and because it would be utterly disastrous to the whole hypothesis if it did not so vibrate! Is there any other reason, real or imaginary, why this one membrane should differ thus from all others If there is, this great investigator does not pretend to point it out, but appears to assume it on general principles. He lays down this remarkable general law:—

"An elastic body set into *sympathetic vibration by any tone* [whether in unison or not], *vibrates sympathetically in the pitch* or *with the vibrational number* of *the exciting tone;* but as soon as the exciting tone ceases, *it goes on sounding in the pitch or vibrational number of its own proper tone.*"—*Sensations of Tone*, p. 215.

There is no difficulty in understanding the drift of this law. It necessarily assumes that a membrane or other elastic body not only has a vibrational number of its own, but will vibrate sympathetically to exciting sounds not in unison with this "*vibrational number of its own proper tone*" so long as the "exciting tone" continues; but that the moment the actuating tone ceases the membrane drops that coerced rate of oscillation, and "goes on sounding in the pitch or vibrational number of its own proper tone"!

Now, this law must evidently apply to the drum-skin of the ear, for reasons which I will give. Professor Helmholtz himself distinctly teaches, as already quoted, that—

"A periodically oscillating sonorous body produces a *similar periodical motion*, first in the mass of the air and *then in the drum of our ear*, and the *period of these vibrations must be the same as that of the vibrations of the sounding body.*"—*Sensations of Tone*, p. 16.

Thus, the "drum of our ear" must oscillate with the same period "as that of the vibrations of the sounding body," whatever may be its pitch of tone or number of vibrations per second,—whether it is in unison with the "vibrational number" of the tympanic membrane, or a thousand vibrations a second out of unison! The drum-skin of the ear, as this writer must include, "*vibrates sympathetically in the pitch or with the vibrational number of the exciting tone; but as soon as the exciting tone ceases it goes on sounding in the pitch or vibrational number of its own proper tone*"! That is, if it "goes on sounding" at all; and, as a proof that the tympanic membrane is thus necessarily included, Professor Tyndall reenforces Professor Helmholtz by distinctly teaching as follows:—

"Every *wave* generated by such vibrations [without reference to pitch] *bends the tympanic membrane once in and once out.*"—*Lectures on Sound*, p. 69.

And to show that this membrane "goes on sounding," bending in and out, after the exciting tone ceases, this same lecturer says:—

"Imagine the first of a series of pulses which follow each other at regular intervals, *impinging upon the tympanic membrane.* It is shaken by the shock; and a body once shaken *can not come instantaneously to rest.*"—*Lectures on Sound*, p. 49.

Hence, as Professor Helmholtz says, "it goes on sounding in the pitch or vibrational number of its own proper tone," because it can not, of course, vibrate out of its normal or unison rate, if at all, any longer than coerced; and, as it can not come immediately to rest after the exciting tone ceases, it must come under this extraordinary law of Professor Helmholtz, and go on sounding in its own normal or "vibrational number."

We will now look at some of the extraordinary and amusing results of this law, as applied to the drum-skin of the ear. Let us suppose a certain tympanic membrane to be of such size, weight, and tension, as to make "its own proper tone" or "vibrational number" that of A, having 440 pendular swings per second; that is to say, if the drum-skin should be thrown into vibratory motion, and left to swing normally, it would continue to vibrate at that isochronous rate till it would settle to rest.

According to the teaching of these physicists,—which we are, of course, expected to believe as science,—if an organ-pipe, representing the highest note but one in a seven-octave pianoforte (G, with 3,400 vibrations in a second,) should be sounded, this tympanic membrane is of necessity coerced from its normal rate of 440 oscillations, and made to assume the vibrational number of this high G, and bend "once in and once out" for each of these 3,400 waves per second, so long as this "exciting tone" continues, though its own pitch or "vibrational number" is only about one eighth as much. But after a little this high G ceases to sound, and instead of the drum-skin of the ear doing likewise, we are assured by these highest living authorities that it "can not come instantaneously to rest," but goes on sounding in the pitch or vibrational number of its own proper tone," or at the old rate of 440 vibrations a second!

Contrary, then, to the observation and scientific experience of the whole world, it is first coerced into an abnormal rate of swing nearly 3,000 oscillations out of tune, and that, too, remember, by "*sympathetic* vibration"; and then, contrary to all known mechanical or acoustical laws, it drops that motion and takes up a new rate of 440 vibrations a second without any known or exciting cause whatever to superinduce it, since we are told that "*as soon as the exciting tone ceases it goes on sounding in the pitch or the vibrational number of its own proper tone*"!

I deny both these positions as preposterously absurd, and contrary to both science and reason. No membrane, however tuned or tensioned, can be excited sympathetically by any tone, as Professor Helmholtz has already admitted, not in unison or very nearly in unison with its own "vibrational number"; and if so excited into an abnormal rate by a discordant sound, it could not change to a new rate without a new exciting impulse.

But the more startling consequences growing out of the doctrine here inculcated have not yet been reached. If this law governing the sympathetic vibration of a stretched membrane or other elastic body —especially the drum-skin of the ear—is correct, as here laid down by these high authorities, we have only to assume, as already intimated, any particular pitch of sound as the one corresponding to the normal "vibrational number" of the tympanic membrane, in order to at once see the beautiful working of the principle enunciated; since it is evident, as admitted by Professor Helmholtz, *that the drum-skin, as well as every other membrane, must have some definite pitch as the "vibrational number of its own proper tone.*"

We have already supposed the pitch of our own tympanic membrane, for example, to be A, or the same pitch as that of the second string of the violin, having 440 vibrations to the second. Now, it is manifest, as just seen, and as I wish again to impress upon the reader, that if D should be sounded, having 594 vibrations to the second, this drum-skin will be instantly forced out of "its own proper tone" and compelled to vibrate sympathetically with D so long as it sounds, according to this remarkable law and the necessities of the wave-theory; but the moment the sound of D ceases, the "drum-skin" drops this abnormal rate of 594 vibrations to the second, and relapses back into "its own proper tone," and "goes on sounding"! Of course, according to this admirable law of Professor Helmholtz, confirmed by Professor Tyndall, the "elastic body *set into* *sympathetic vibration*" by the sound of D does not cease sounding or "come instantaneously to rest" when D ceases, though it ceases sounding in the pitch of D, or with 594 vibrations to the second, but "goes on sounding" in A, with 440 vibrations, for "as soon as the exciting tone ceases it goes on sounding in the pitch or vibrational number of its own proper tone"!

Thus, inevitably, if these writers are received as authority,—and they confessedly stand the highest on this subject,—it follows that on the cessation of every sound we hear, either above or below A, the ear instantly reverts to "its own proper tone," and "goes on sounding" in A! Hence, A must be sounding in my ear all the time as a perpetual *monotone* while an orchestra is playing, filling up every interval which occurs in any piece of music I hear. No matter what may be the *pitch* or the vibrational number of the exciting tones, if there is not a single A sounded by the entire orchestra, the tympanic membrane must instantly *jump* to the tones they produce or *fall* to them by "sympathetic (!) vibration," and continue to oscillate at that abnormal rate per second till such "exciting tone ceases," when, as before observed, it *falls* back or *leaps* back, as the case may be, to "the pitch or vibrational number of its own proper tone," and "goes on sounding"!

Thus, while the drum-skin "can not come instantaneously to rest," but "goes on sounding" A, at 440 vibrations a second or 'its own proper tone," these accurate scientists and greatest living authorities on sound tell us if some one in the orchestra should strike the high D of the piccolo-flute, with 4,752 vibrations in a second, the drum-skin of the ear temporarily ceases sounding A, on which it is vibrating when not coerced, and leaps a distance of 4,312 oscillations a second out of unison or away from *sympathy*, and continues to keep up this rapid, abnormal, coerced movement, by "sympathetic vibration," so long as the piccolo-flute sounds that note! Or, if the low E of the double bass should happen to be struck, with 40 vibrations to the second, the tympanic membrane (which is now supposed to be filling up the interval, after dropping from the high D of the piccolo-flute, by sounding A, "its own proper tone,") is instantly forced down to the "vibrational number" of this new "exciting tone," and is thus compelled to swing at this slow rate of 40 oscillations a second by "*sympathetic* vibration," or just 400 swings a second out of tune or away from *sympathy!*

The result is, in listening to an orchestra of fifty pieces, we not only hear A all the time, filling up all the intervals between the countless myriads of notes of various degrees of pitch, but we hear fifty A's at one time, making each instrument appear to sound in our ear practically like a demoralized hurdy-gurdy, and converting the orchestra into an enormous band of Scotch bagpipes, with their everlasting droning and monotonous A continually ringing its changes upon our tympanic drum-skin!

But the foregoing is not all there is in this lucid principle which controls the "sympathetic vibration" of this membrane of the ear, as announced by these eminent physicists. It is well known that a musical instrument, when re-enforced by the sympathetic resonance of another sounding body which vibrates in unison, is *louder* than it would be if not so re-enforced,—while the unison instrument, which sounds alone by sympathetic vibration, must necessarily be *vastly louder*, as every one knows, than it would be if coerced into an abnormal vibration by a discordant tone,—that is, if such abnormal oscillation were possible, which it manifestly is not. Professor Helmholtz, however, as shown in the last citation, claims it to be possible, as he is, of course, compelled to do to make it possible for the "drum-skin of the ear" to vibrate sympathetically to tones of every degree of pitch, though he does so in defiance of the experience and observation of the whole scientific world. But suppose we admit it to be true, for the present, that this drum-skin of the ear is sufficiently accommodating to the necessities of the wave-theory to act unlike all other membranes, and to thus contradict all observation; yet it is nevertheless undeniable that when the note A should happen to be sounded the tone would be enormously louder than when any other note not in accord was heard, because the drum-skin, being thus in sympathetic unison, would surely oscillate with many times greater amplitude and force when sounding in "the pitch or vibrational number of its own proper tone"; because this tone, according to Helmholtz, is so easy and natural to make that the drum-skin "goes on sounding" it without being excited into action by any tone whatever! It simply *jumps* or *falls* into it without the least effort! But this does not require an argument. It is self-evident; and Professor Helmholtz would instantly admit that the tympanic membrane would vibrate with vastly greater amplitude in sympathetic response to a *unison* note than to a *discord*.

Then it follows, with my "drum-skin" tuned as I have supposed, that in listening to an orchestra, the one single note A, whenever struck by any instrument, would always appear immensely louder to me than any other note, not only because it would produce greater vibratory motion in my ear, but because it would be sure to meet with re-enforcement by this continual relapsing of the membrane at the end of every other note, as "*it goes on sounding in the pitch or vibrational number of its own proper tone.*" Hence, in my case, with my drum-skin tuned as supposed, A would always be the predominant tone, and enormously louder than any other sound I could hear; that is, if there is any truth in this hypothesis of tympanic vibration, which I am controverting.

But even this is not the funniest feature of the problem. As the "vibrational number" of any stretched membrane depends on its *size*, *weight*, and *tension*, and as it is perfectly evident that no two "drum-skins" would combine these elements to exactly the same degree in different individuals, it follows that with one person A would be the predominant or loud note, with another B or B♭, with another C or C♯, with another D, and so on through the chromatic scale, or possibly through several octaves,—the smaller the person and the younger the child the higher the pitch of the note would become which would sound the loudest, and *vice versa!*

Thus, while A would be to me a very loud sound, being in sympathetic accord with the "vibrational number" of my tympanic membrane, B, C, D, E, F, &c., would be comparatively but feeble tones, whatever the *vis viva* in their production; whereas Professor Helmholtz, being a larger man, would probably have a "drum-skin" tuned to G, which, in turn, would make it the loud tone to him, while he should scarcely be able to hear A, or any other note of the scale, according to this advanced scientific hypothesis, since such rates of vibration in his ear would have to be coerced by a discordant tone! In this way no two persons would be physically able to estimate the same tone as having the same degree of intensity, owing to the intrinsic and constitutional diversity in the "vibrational numbers" of their respective "drum-skins,"—depending, of course, on their size, weight, and tension! A theory based on such a sapient hypothesis as this, and supported by such trustworthy authorities, surely ought to command the respect of the great intellects of the world!

But this theory of tympanic vibration is self-destructive in more ways than one, as I will now undertake to show. Physicists assume sound and light to be every way analogous, and both to be equally the result of wave-motion,—the former acting on the auditory nerve by means of *air*-waves and their impression on the tympanic membrane, while the latter acts on the optic nerve by means of *ether*-waves and their impression on the *retina*. No man will dispute this statement who has any knowledge of the undulatory theory of light, and the arguments by which that hypothesis has been deduced from the supposed atmospheric waves of sound.

Hence, if it can be proved that *ethereal* undulations do not and can not convey the impressions of light to the optic nerve, and through it to the brain, by the vibratory motion of the *retina*, it must establish, by necessary analogy, that the impressions of sound are not produced on the auditory nerve, as physicists claim, by the oscillations of the tympanic membrane. Is not this logically and necessarily evident?

That the *retina*, corresponding to the *drum-skin* of the ear, can not transmit the impressions of light to the optic nerve by oscillating in synchronism to the waves of *ether*, will strike every intelligent reader as self-evident the moment we consider how many times this sensitive organ would be obliged to actually and mechanically *swing to and fro* every second to equal the periodicity of these supposed *waves of ether*. If the reader is not posted on this special question, it would be impossible for him to make even an approximate guess.

Let us consider this matter for a moment. The highest sound in music is generated by only four or five thousand vibrations in a second, which physicists have mistakenly supposed to be transferred by a corresponding number of air-waves to the tympanic membrane, producing a corresponding number of oscillations of that organ. But thousands of vibrations a second are absolutely as nothing when it comes to the inconceivable number of swings the *retina* must make to and fro as the waves of *ether* strike it! Millions of such oscillations a second are nothing! Hundreds of millions are nothing! Thousands of millions are nothing! Hundreds of thousands of millions of such swings, in and out, of this delicate sensitive organ every second are but as the drop to the bucket contrasted with the actual number of times the *retina* has to oscillate, if it acts in accordance with the teaching of the wave-theory of sound, and vibrates as this drum-skin is forced to do. This is no exaggeration, if there is any analogy between the modes of propagation of sound and light, and if wave-motion in both cases is, as universally taught, the correct solution of their phenomena.

Professor Tyndall distinctly teaches that no less than *six hundred and ninety-nine million million waves of ether have to strike the retina every second while we are looking at a violet light!* These are his words:—

"All these *waves* enter the eye *in a second.* In the same *interval* 699,000,000,000,000 *waves of violet light enter the eye. At this prodigious rate is the retina hit by the waves of light.*"—TYNDALL on *Light*, p. 66.

Thus the *retina*, or this analogue of the tympanic membrane, if there is any truth in the theory of wave-motion, must physically and mechanically bend "once in and once out" as each wave of light hits it, or, as here authoritatively given, must actually oscillate to and fro 699,000,000,000,000 times every second without producing the least injury to this most sensitive and delicate organ!

Is it possible for an intelligent man to believe that a physical organ of any kind could exist for a single second unimpaired, even if constituted of material a thousand times more durable than the finest steel, subjected to this process of being thus bent "once in and once out" as many times a second as required by this insane hypothesis? If not, then retinal oscillation is proved to be an absolute chimera, and with it tympanic vibration also breaks down, since modern science assures us that the two operations are entirely analogous, and equally depend upon wave-motion for their sensations.

If, to avoid this manifestly destructive effect on the *retina*, by thus bending in and out 699,000,000,000,000 times a second, it should be denied that any physicist claims such a preposterous result, or supposes it possible that the *retina*, being a physical, ponderable body, can be stirred at all as the effect of contact with an incorporeal substance like *ether*,—then I answer, if *light* can make its appropriate impression on the *retina*, and if this organ can transmit all the complex sensations of tints and shades of color to the optic nerve, and through it to the brain without the aid of retinal oscillation by the dashing of *ethereal* waves, why, in the name of science and reason, can not its congener—the drum-skin of the ear—receive and then transmit its characteristic impression to the auditory nerve in the same way, and without any oscillatory motion whatever?

Thus, in every way the question is viewed, tympanic vibration is rendered as useless as it is impracticable. It does not require a philosopher to see at a glance that if both *light* and *odor* can produce their appropriate and peculiar impressions on their special nerves of sense without bending in and out the *membranes* with which they first come into contact, that the oscillation of this sensitive membrane of the ear would not only be analogically unnecessary, but an abrupt departure from the order, uniformity, and harmony of Nature's plans. It would seem that no other argument would be required to overthrow this impracticable assumption of tympanic vibration save this single class of analogical facts just referred to, especially in view of the undulatory theory of light, which has been alone deduced from the supposed action of sound.

Really, this question of tympanic vibration as the effect of sound, on which the wave-theory absolutely rests, needs only to be presented in its proper light to a mind capable of reasoning philosophically on any question of science, to show its entire uselessness as well as impracticability. The bare fact that such pretended laws and principles as those recently examined, by which a membrane may be forced to vibrate *sympathetically to tones of every conceivable pitch*, have to be employed in order to give a show of plausibility to this vital assumption of tympanic oscillation; and the simple consideration that renowned physicists, like Professors Tyndall and Helmholtz, are compelled to resort to such a preposterous fallacy as that any musical instrument will vibrate "*sympathetically*" to a pitch of tone 4,000 oscillations out of unison, and that as soon as such exciting tone ceases will relapse to its normal swing, and go on "*sounding in the pitch or vibrational number of its own proper tone*," as the tympanic membrane must necessarily do, ought to be enough to condemn the hypothesis in the estimation of every logical mind, even if it had not been demonstrated, as recently done, that such vibration mechanically involves the displacement of *two thousand million tons* of ponderable matter *four hundred and forty times a second* by the physical strength of an insect!

But I am even yet not through with this unspeakable folly of tympanic vibration. Its impracticability is so unavoidably self-evident that it is impossible for Professors Tyndall and Helmholtz to touch this question without developing the most startling and glaring inconsistencies. For example, in explaining "Corti's arches,"—a mass of microscopical processes in the inner ear,—they account for the use of these numerous rods or fibers as they bristle around the appendages of the auditory nerve, by assuming that they serve the practical purpose of conveying sounds of *different pitch* to the brain by each of the different arches vibrating *sympathetically* or in "unison" with the *corresponding pitch of tone* as it strikes the drum-skin of the ear! Thus, each individual arch or rod of Corti, having a proper vibrational number of its own, can only respond when a "unison" sound, or one nearly of a corresponding vibrational number strikes the tympanic membrane!

Notwithstanding its utterly suicidal and subversive character, involving as it does the flattest possible contradiction of the idea that the "drum-skin" of the ear can vibrate *sympathetically* and with equal facility to every audible pitch of tone, yet these greatest of modern physicists and the leading sound experts and investigators of the world go on innocently fabricating their theory of Corti's arches and their absolute acoustical necessity in the mechanism of the ear for the transportation of *each separate pitch of tone* to the brain by the *sympathetic* vibration of a correspondingly tuned Corti's arch,—forgetting, as usual, for the time being, that this single little drum-skin of the ear, a third of an inch in diameter, can individually and alone take on as many different vibrational numbers and respond sympathetically to as many separate degrees of pitch as the whole of Corti's 3,000 arches put together, where there are, as we are told, about *fifty* rods tuned in unison for each tone of the audible register!

The whole matter is thus so pitiably self-stultifying and subversive of the fundamental principles of the wave-theory, as based on tympanic vibration, that I must treat the reader to a brief citation or two. Professor Helmholtz remarks:—

"When a simple tone is presented to the ear, those Corti's arches which are *nearly or exactly in unison with it will be strongly excited*, and the rest only *slightly or not at all*. Hence, every simple tone of *determinate pitch will be felt only by certain nerve-fibers, and simple tones of different pitch will excite different fibers*. When a compound musical tone or chord is presented to the ear, *all those elastic bodies will be excited which have a proper pitch corresponding to the various individual simple tones contained in the whole mass of tones;* and hence, by properly directing attention, all the individual sensations of the individual simple tones can be perceived."—"The end of every fiber of the auditory nerve is connected with *small elastic parts*, which we can not but assume to be *set in sympathetic vibration by the waves of sound*."—*Sensations of Tone*, pp. 190, 222.

In addition to these statements, on page 218, in speaking of the same rods of Corti, he insists that they "*must be differently tuned, and their tones must form a regularly progressive series of degrees through the whole extent of the musical scale*,"—even, of course, down to the lowest notes of the pianoforte or organ!

Professor Tyndall is equally explicit in teaching that Corti's organ must be an instrument having its multitudinous strings tuned in "unisonant vibration" with all our audible musical sounds:—

"Finally, there is in the labyrinth a wonderful organ, discovered by the Marchese Corti, which is to all appearance a *musical instrument, with its chords so stretched as to accept the vibrations of different periods and transmit them to the nerve filaments which traverse the organ*. . . . Each musical tremor which falls upon this organ selects from its *tensioned fibers* the one appropriate to *its own pitch*, and throws that fiber into *unisonant vibration*."—*Lectures on Sound*, p. 224.

These quotations only need to be casually examined for the reader to recognize the complete absurdity of this entire assumption, so essential to the wave-theory, namely, that the tympanic membrane, singly and alone, tuned necessarily to one single pitch, if tuned at all, can take on a vibratory motion corresponding to every sound we hear, whatever may be its pitch.

We must understand that Corti's arches are located in the labyrinth between this tympanic membrane and the brain, and that every sound we hear has to first pass through the drum-skin, according to this theory, by the proper vibratory motion, before it can play upon this harp of three thousand strings! According to Professors Helmholtz, Tyndall, Mayer, and, in fact, all writers on sound, this one little membrane can not only vibrate by the synchronous dashing of air-waves in perfect periodicity to every pitch of tone we hear, assuming each separate vibrational number, but it can even oscillate to fifty or a hundred or even a "thousand" different degrees of pitch at once! But as soon as the sound passes through this membrane, which alone answers the purpose of oscillating to every shade of pitch we hear, it absolutely requires a separate Corti's arch of the exact "unison" length and tension for each separate pitch, in order that high and low sounds may be equally conducted to the brain! Why, in the name of acoustics and common sense, can not a single Corti's arch, of a single length and of one degree of rigidity, vibrate to all possible pitches of tone, when a single diminutive drum-skin is susceptible of taking on not only a suitable rate of vibratory motion for every audible tone throughout the musical scale, but can adapt itself to a "thousand" different and antagonistic vibrational rates at one and the same time? The pitiable involvement of the wave-theory becomes more and more conspicuous and hopeless at every new advance made in the examination of its details.

Another practical absurdity in the assumed sympathetic vibration of Corti's rods, "*differently tuned*" to respond to tones of all degrees of pitch, or "*through the whole extent of the musical scale*," as just quoted, must strike the critical reader at a glance. The "differently tuned" strings of a pianoforte, in order to produce its seven octaves, are not only compelled to vary in length from $5\frac{1}{2}$ feet to $1\frac{5}{8}$ inches, the difference being as 1 to 40; but the size and weight of these strings, from the lowest to the highest, must diminish in about the same proportion. Thus, there is a difference between the *weight* of the highest and lowest strings of the pianoforte, in order to "*form a regularly progressive series of degrees through the whole extent of the musical scale*," as 1 to about 1600!

How is it, now, with these Corti's rods, which, as Professor Helmholtz claims, accomplish the same acoustical result, and which Professor Tyndall describes as a "*musical instrument, with its chords so stretched as to accept the vibrations of different periods*"? The fact is well ascertained by Hensen's careful measurement, which was right before the eyes of both Professors Helmholtz and Tyndall when they made these statements, that the difference of length between the longest and shortest of these rods is only about *one half*, or as

1 to 2, while no perceptible difference in size is recorded Notwithstanding this essential and patent acoustical fact, these model investigators, either ignorant of its bearing on the main question or regardless of the scientific opinions of mankind, ignore it as if it had no existence, and go on bunglingly to teach that these microscopical rods, with only this maximum difference in length as 1 to 2, and no difference in thickness, are actually tuned as a "musical instrument" of 3,000 strings, in absolute "*unison*" with the chords of a seven-octave pianoforte, having an unavoidable difference in length, in order to generate the tones, as 1 to 40, and a necessary difference in weight as 1 to 1600! Yet such teachers and such instruction are pointed to as the highest "scientific" authority on sound!

I must ask the reader's indulgence while presenting just one other and the closing argument against this vital assumption of the wave-theory that the tympanic membrane or Corti's rods can vibrate, by any possibility, in "unison" with musical sounds,—an argument, by the way, which, like the preceding, admits of no kind of reply.

The truth is, no argument would be really necessary to show the practical impossibility of any such an operation as tympanic vibration, or the "unisonant" response of Corti's rods, to a mind possessing the least original scientific capacity. I say this advisedly and deliberately, but kindly. It is only for these so-called *scientific investigators*, who have learned to circle in this beaten theoretic path, that any serious argument is required,—who, however competent and profound on other questions of science, seem so completely bewildered and blinded by the influence of the wave-theory of sound, that they exhibit the puerility of mere children the moment they come to treat of the effects of wave-motion upon the ear, and the office of its individual parts.

This charge, I admit, appears supremely ridiculous on its face, made against such world-renowned scientists as those I am reviewing; but, after the most careful deliberation, I defy any man of ordinary intelligence to doubt the exact and literal truth of the impeachment, after paying the slightest attention to the arguments here being presented. The reader need go no further for the evidence on which to base his decision as to its correctness than the single consideration which I will now submit.

As surprising as it may seem, these learned authorities, who have devoted much of their lives to the investigation of sounding strings, reeds, forks, rods, membranes, &c., and who have experimented hundreds and perhaps thousands of times on the proper length, weight, and rigidity of strings, and size and tension of membranes to produce tones of certain determinate degrees of pitch, have never once taken the trouble to think of the practical impossibility of rods or strings under a certain definite length, weight, and rigidity, producing such results, or responding to them, by "unisonant vibration"! With all their experience and familiarity with such phenomena, it never occurs to them, when they come to philosophize about the individual parts of the ear, and when trying to adapt them theoretically to the chimerical requirements of the wave-theory, that it is acoustically essential for a string to be at least of a certain determinate length in order to vibrate in "unison" to the low notes of the pianoforte, for example, but really suppose and seriously publish to the world that a Corti's *rod*, only the one 300th of an inch long (less in length than the diameter of a common hair), is capable of vibrating in "unison" with, and hence of actually producing the tone of, the low A of the pianoforte, having but twenty-seven vibrations to the second,—which, under the best mechanical skill, requires a string with a length of about five feet, and a weight at least of several ounces!

Instead of allowing this essential feature of *length*, *weight*, and *rigidity*, a place in their thoughts, as a basis for determining the "vibrational number" of a given string, or other sounding body,—the very first thing a schoolboy would take into account, if his attention were called to the subject, —they quietly and innocently ignore this whole question, as if it had nothing to do with the laws of acoustics, and go on reasoning about a loosely stretched membrane, a *third of an inch in diameter*, having the same vibrational number as that of the head of a bass drum, *with a diameter of three feet!* Is not the charge I have just made well founded? Let us illustrate the matter in a way which can not fail to produce conviction.

Imagine Professor Helmholtz stepping into the pianoforte manufactory of Mr. Steinway, in this city, where he finds the proprietor busily engaged on an improved working model of a grand piano, *about an inch long!* I can fancy the following conversation as occurring between this greatest of living acousticians and sound experts, and this king of pianoforte-makers.

HELMHOLTZ.—"Good morning, Mr. Steinway. What in the world are you making there, in which you seem to be so deeply absorbed?"

STEINWAY.—"A grand piano, sir;—an improvement that is going to revolutionize the business, based on late acoustical discoveries which do away with the necessity of such enormous size and expense in construction. I am building, sir, a vest-pocket piano,—one that a musician can carry with him, wherever he goes, as easily as he can carry his watch. 'There are millions in it!'"

HELMHOLTZ.—"What length, Mr. Steinway, do you propose to have the strings?"

STEINWAY.—"The longest strings, or those producing the lowest notes of the bass, according to my improved scale, which I have just completed, will be exactly *one inch* in length, while, for the highest notes, seven octaves above, the strings will be just *half that length.*"

HELMHOLTZ.—"Mr. Steinway, you are a practical joker. But come, now, be serious. We Germans do not deal in jokes when we come to mechanical improvements, involving, as yours does, the established laws of acoustics,—especially when our knowledge of them harmonizes with the universal experience of acousticians and musical instrument makers. You surely can not be in earnest about practically producing the tones of the pianoforte on such a diminutive affair as the one you are constructing!"

STEINWAY.—"I am in earnest, sir; and you will find, before you are through with me, that it is anything but a '*joke.*' I am prepared to prove that the laws of acoustics have always been misunderstood until very lately, and that musical instrument makers have all been laboring under a foolish and expensive mistake in regard to the length of strings essential to generate the low tones of a pianoforte, since it is now demonstrated by recent scientific discoveries that strings *an inch long* are even more than sufficient for the lowest bass notes of the musical scale. You smile, sir, and seem astonished; but you will find that this valuable improvement, based on scientific principles, is anything but a 'joke.'"

HELMHOLTZ.—"Why, my dear sir, you are crazy! Your constant study over this instrument for so many years must have turned your head, and converted you into a *monomaniac* on the question of improving the pianoforte! Take my advice, and burn your model at once; and banish the hallucination from your thoughts. It will ruin your reputation and your business, as it is all nonsense, and a clear evidence of insanity in your case, to suppose that you could generate as low a note as A, with twenty-seven vibrations in a second, on such diminutive strings as those on your model, only an *inch long* and no *thicker than fine silk threads;* and then it is worse than folly that you should suppose it possible to raise the scale through *seven full octaves* by a reduction of only *one half in their length*, when the laws of acoustics, according to all experience, require the bass strings of a pianoforte, in order to generate the appropriate tone, to be *over five feet long*, and the length of the highest strings, for seven octaves above, to be but the *one fortieth* as much! Yet you madly essay to accomplish the same result, with a difference of only *one half!* I am surprised that you could ever have permitted such a baseless fallacy to take possession of your thoughts! Why, Mr. Steinway, the idea of attempting to make a string only *an inch in length* assume the normal swing or vibrational number of one *five feet long*, surpasses in folly the whimsicality of the clockmaker who would attempt to force a *pendulum* to beat *seconds* with a rod no longer than one of your strings. Think of it! A child, half a dozen years old, ought to know better than this!"

STEINWAY.—"Professor Helmholtz, I will give you the reasons which have led me into this important improvement. I have been reading lately a couple of popular works on acoustics and sonorous phenomena in general,—one called the *Sensations of Tone* and another called *Lectures on Sound.* In these able productions I have learned, for the first time, to my surprise, that Corti's microscopical rods, situated in the labyrinth of the ear, constitute a 'musical instrument'—a 'lute of 3,000 strings'—which is actually tuned in 'unison' to all the different strings of the pianoforte, from the lowest bass notes up to the high A of the upper octave. And I also found, in these popular and authoritative scientific works, that there was only a difference of *one half* between the length of the longest and shortest of these Corti's rods, which has led me to improve my scale accordingly. But, most important of all, I found that the longest of these rods was only about the one 300th of an inch in length, and that this rod really oscillated in 'unisonant vibration' to the lowest note of the piano. Why, then, should you call me crazy, and seem so astonished because I take advantage of this important scientific discovery, especially *when the strings on my model are exactly three hundred times longer than are the strings of this wonderful 'musical instrument' in the human ear, which responds sympathetically by 'unisonant vibration' to every note of a grand piano?* You evidently are not posted in modern science; for, if you had read these standard works on sound, you would have applauded my advanced ideas as away ahead of all competitors in the art of pianoforte-making, instead of charging me with being a 'monomaniac'!

"I admit, at once, that the pendulum is governed by the same isochronous law; and hence I assume that clockmakers, as well as pianoforte-makers, have always labored under a radical misapprehension, for *science* can not be wrong, of course; and therefore, according to these recent acoustical discoveries, it is perfectly manifest that no special length of rod is needed to produce sixty or any other number of oscillations of the pendulum-ball in a minute! I intend, as soon as I have demonstrated the correctness of my piano-scale, to go and see the clockmakers of this city, and bring about a revolution in their crude ideas of the pendulum and the length of rod necessary for determinate rates of oscillatory motion.

"I fear, my dear sir, that it is the authors of those books on sound who are insane, *or at least just three hundred times nearer being monomaniacs than your humble servant.* Whenever those books of which I have spoken (which teach that strings and rods three hundred times shorter than those of my instrument can be tuned to vibrate in 'unison' to every note of a grand piano) shall be made a public bonfire of, as an oblation to the cause of true scientific progress, you can then ask me to burn my model,—not before. Good-day."

Really, with such a practical rejoinder as this, one can imagine Professor Helmholtz making a bee-line for Berlin to destroy his stereotype plates and revise his *Sensations of Tone*,—while he no doubt would stop off on the way in London, and suggest to Professor Tyndall the propriety of adopting a similar course.

It would seem that the infinite impossibility of one of Corti's rods actually vibrating in "unison" with the E-string of the double bass, for example, or with any other note in the audible register, would be so self-evident that its suggestion and advocacy in any work on science would be scouted and laughed at, and its author branded by universal acclamation either as a scientific lunatic or an ignorant pretender. Yet, instead of this, the very works which teach such inexpressible nonsense as this "unisonant vibration" of Corti's rods to every tone of the musical scale, are received as standard authorities in our greatest institutions of learning.

If these microscopical rods of Corti can really vibrate at all in "unisonant" response to tones of any kind, it is perfectly evident that *such tones must also be microscopical;* that is to say, the tone which would be adapted to the excitation of such a rod would require to be as much *finer* and *higher* than ordinary musical sounds as these strings of Corti's organ are more diminutive than those of ordinary musical instruments! Is not this acoustically rational and consistent? Then, as these rods of Corti are but the one 4,000th as long as the strings of the violin, for example, it follows that Corti's "lute of 3,000 strings," as Professor Tyndall calls it, ought only to respond by "unisonant vibration" to a tone 4,000 intervals higher than those generated on the unfingered chords of the violin! This must be obvious to every thinker.

A church-organ builder who should become so demented or infatuated with modern science as to attempt to substitute for his longest pipe a section of a timothy straw an inch in length, expecting thereby to produce the same result, though he would be pronounced a monomaniac by Professor Helmholtz, as was the case with the piano-maker just supposed, is really three hundred times *less insane* than the scientific writer who insists that a Corti's rod the one 300th of an inch long is capable of vibrating in "unison" to the same pitch of tone. Yet these learned authorities can not see it.

But, finally, to cut the argument short on these Corti's rods, and thus brush the whole hypothesis of the "unisonant vibration" of this "lute of 3,000 strings" out of existence at a single sweep, it is only necessary to refer to the recent discovery of C. Hasse, by which he has shown that these microscopical processes, so essential to the wave-theory of sound, have no existence at all *in the ears of birds!* Yet it is a notorious fact that the mocking-bird can distinguish and analyze tone, noting and imitating the finest shades of difference in *pitch*, equal to a prima-donna! Thus, we have at last a fitting culmination to one of the most stupid and inexcusable scientific fallacies of this or any other age.

If Professors Helmholtz and Tyndall *have* been blindly led into this fatal assumption of tympanic oscillation and the "unisonant vibration" of Corti's rods in response to the lowest strings of the pianoforte, they are neither of them so stupid as not to realize, as soon as they read this exposure, the doom which has overtaken their elaborately developed hypothesis. To suppose that such renowned investigators of sonorous phenomena do not know and can not see, when they come to reflect, that such "unisonant vibration" and tympanic oscillation are out of the question, and acoustically impossible and absurd, would be to proclaim them ignorant of the elementary principles of science. Yet that *they did not know it when they wrote their works on sound*, but actually believed a locust capable of shaking millions of tons of physical drum-skins by the motion of its legs, and that the infinitesimal rods of Corti were actually tuned so as to vibrate in "unison" with the lowest notes of the piano and church-organ, is conclusively shown by the numerous quotations from their works already made. What explanation they can make, if any, remains to be seen. I venture the prediction that no reply to these ruinous arguments will ever be made or even attempted.

Really, in view of such mechanical and acoustical fallacies, publicly taught in books and lectures, and which everywhere superabound in the writings of these physicists, gravely spread out before the world as *philosophy* and *science*, and which a schoolboy might easily have known to be without a possible foundation in fact, one is almost inclined to doubt *in toto* the advantages of a scientific education, and to fall back, as the only safe thing, on the common schools of our ancestors. What is the use, one is tempted to ask, of our so-called "scientific courses," in colleges and universities, which lead to such preposterous results?

We need no better illustration than the one before us, since we can scarcely imagine it possible, in this seventh decade of the nineteenth century, that any physicist or mathematician could be found who would venture to teach that the tympanic membrane actually bends "once in and once out" for each sound-wave and for every audible pitch of tone we hear, without regard to "vibrational number"; or that Corti's rods, less in length than the diameter of a hair, can be actually tuned in "unison" with the strings of the violoncello!

Still, the fact that such unspeakable absurdities in science are really taught by sound experts and investigators, like those from whom I am quoting, must be attributed alone, as I have already explained, not to their want of intellectual ability or scientific culture and discrimination, but to the paralyzing and blinding influence of the prevailing theory of sound. But even this fact, that a few such specialists should be thus misled and duped by a universally accepted theory, to which they have devoted much of their lives, is not nearly so surprising as that the same fallacies should be adopted and believed by scientific thinkers throughout the land, and of all classes, without one man being found to lift his pen or his voice against such an imposition upon the education of the world.

I have felt, at times, while plodding through these learned disquisitions on the tympanic membrane bending "once in and once out" by the contact of air-waves which have no existence, and of Corti's rods, which have no necessity for moving at all, being *tuned* to "unisonant vibration" with the strings of the double bass, that if the earnest and sincere manner in which the positions were maintained did not preclude derision by evincing such intense candor on the part of these writers, the hypothesis ought justly to meet with the jeers and laughter of the whole scientific world. As it is, the hypothesis from beginning to end appears to the writer like a serious scientific joke, too absurd to believe and yet too grave to laugh at.

But I have pursued this feature of the subject farther than I had intended; and sufficiently, I trust, to convince the reader that the vibratory motion of the tympanic membrane, as well as of Corti's rods, is purely visionary, without the least foundation in fact or necessity in science, being impossible in the nature of things, and self-contradictory, as we have seen, even in the hands of the most careful and critical advocates of the wave-theory of sound.

I repeat, and emphasize it, and wish to impress it on the mind of the reader, that if the *retina* can receive the supposed waves of *ether* in countless millions per second, and transfer their impression to the optic nerve without any oscillatory motion whatever of that sensitive organ, and if the *membrane of the nose* can receive by direct contact the admitted corpuscles of odor and convey their impression to the olfactory nerve, along which it is conducted to the brain, and there analyzed and translated into its characteristic sensation of smell, without the intervention of any kind of wave-motion of *air* or *ether*, and without any vibratory action either of the nose or its membrane, then what absolute folly and waste of valuable time on the part of Professors Tyndall and Helmholtz is all this labored and contradictory effort through hundreds of pages of their books to prove that we only hear sound by means of the oscillation of the tympanic membrane or the "unisonant vibration" of Corti's arches!

What conclusion, then, are we to come to as regards the true cause of these over-tones, resultant tones, &c., from which I have unavoidably been forced to digress in order to examine thoroughly this question of tympanic vibration? They can not result from the "vibrational form" assumed by a string while oscillating as a whole, and thus producing its fundamental tone, as it would require the string to divide itself up into as many as eighteen different sections in addition to the primary, some of them not much over an inch long, and each section to take on a separate and independent rate of vibratory motion corresponding to the pitch of its special over-tone. This, without an argument, must strike the mind as an utter impracticability.

The assumption of Professor Helmholtz that the "vibrational form" of a violin-string under the action of the bow is the real cause of the peculiar quality of such tone, and consequently the cause of the ten over-tones thus generated which constitute such quality, and which can be heard in connection with its primary tone, is entitled to but very little weight in the estimation of the reader. It will be recollected that while originally preparing his hypothesis of "vibrational form," and describing the peculiar manner in which the string oscillates and its velocity in relation to that of the bow, he perpetrated one of the most ridiculous and inexcusable scientific blunders on record, making the normal velocity of the oscillating string *ten times greater than that of the bow in the player's hand!* I refer the reader back to that memorable *trip-hammer* fiasco exposed on pages 95–98, in which it was shown that his whole hypothesis of vibrational form was based on an assumed state of facts which turned out to be exactly in every respect the opposite of what he supposed. If, therefore, this eminent investigator, in laying the foundation for his hypothesis of "vibrational form" as the true solution of the cause of over-tones, is wildly at sea on its fundamental element, a matter which a child a dozen years old should have understood, ignoring and misconceiving the primary and governing laws of physics as he did, is it not more than probable that he has also misapprehended the other essential features of these phenomena? At all events, though I make it a rule to attribute all these errors to the blinding influence of the wave-theory, it may be considered every way safe, nevertheless, not to rely too implicitly on the absolute accuracy of observations which have shot so wide of the mark as in the case referred to, and which have been also found wanting in so many essential instances as pointed out all through the preceding argument.

But even supposing that the violin-string could take on *eleven* separate vibrational rates of motion, acting like the trip-hammer in the mill, rising with the bow slowly and then returning *ten times* as rapidly, I have already shown that the eleven separate systems of air-waves. necessary for the propagation of these over-tones, according to the wave-theory, do not and can not exist, whether superimposed or not; and if they did exist, they could not produce eleven systems of oscillation in the tympanic membrane, since that organ does not vibrate at all in response to sound, and is not so intended to vibrate, as demonstrated in half a dozen ways. And, finally, I have shown from Corti's arches the unspeakable folly of this whole vibratory hypothesis as relates to the ear and its individual parts as the means of conveying sound to the auditory nerve, and through it to the brain.

In view of all these considerations the reader must admit the probable correctness of the conclusion that these overtones are neither generated by the eleven-fold vibrational form of the string, propagated by the eleven-fold superimposed systems of air-waves, nor transmitted to the brain through the eleven-fold vibrational movement of the tympanic membrane.

The wave-theory, then, being shown to be wholly inadequate to explain the cause of these phenomena, or to account in the slightest degree for their manner of propagation or transmission to the brain through the sensitive mechanism of the ear, let us now see if the corpuscular hypothesis may not furnish a rational clue to the solution of over-tones. If it shall turn out, after a careful examination of the question, that the assumption of substantial sonorous pulses really meets and solves this complex and difficult problem as beautifully and consistently as it has met and explained other phenomena encountered since the commencement of this investigation, without rippling the surface of the solution with a single contradictory or impossible detail, it would then seem little short of downright madness, not to say pig-headedness, on the part of physicists to reject the possibility of corpuscular emissions, and cast them aside as unworthy of scientific consideration.

On the assumption that sound, like odor, is really a substance of unknown but wonderful attenuation, emanating from the sounding body in absolute corpuscles, there would be nothing at all unreasonable or marvelous in the fact that primary sonorous particles, generated by the vibratory motion of the string, should, on radiating through the air, scintillate or give birth to secondary systems of corpuscles, which might pass off in pulses not only of the periodicity of the primary radiations, but which might include many different vibratory rates corresponding to and thus producing the feeble over-tones of different degrees of pitch described by Professor Helmholtz as heard accompanying the fundamental sounds of instruments.

This explanation of over-tones, resultant tones, &c., as their probable solution, and the most rational way of accounting for the quality of tone, was distinctly foreshadowed while discussing the decrease in the intensity of sound as the square of the distance from its source. (See pages 156, 161.)

By turning back to this reference it will be seen that the primary corpuscles of sound may not only become radiating centers for other systems of smaller sonorous particles, but that these in turn may likewise become radiating fountains of still smaller offshooting systems, and so on,—each new system of radiations, or at least a portion of each system, passing through the air with such relative periodicity as will correspond exactly to the vibrational numbers of the over-tones heard, the same as if they had been generated as harmonics by the vibratory motion of corresponding ventral sections of the string.

In this way the over-tones resulting from successive subradiations would necessarily become fainter and fainter about to the same degree as observed; and instead of being limited in number to the producible and audible harmonics of a string, or even *eighteen*, as noted by Professor Helmholtz, we might reasonably suppose that the constantly diminishing systems of radiating corpuscles might be extended far beyond the power of human observation, the ear in the mean time being only capable of recognizing, by the best scientific helps, the number already indicated. The probability of such an almost unlimited extension of these higher and fainter over-tones only adds to the absolute impossibility of accounting for their generation by the unlimited multiplicity of segmental divisions of the string, or of their propagation by an equally complex superposition of atmospheric undulations.

Although this hypothesis of secondary radiations of sonorous corpuscles, as the actual cause of over-tones, can not be directly demonstrated, it is equally true that it can not be disproved, as has been done in the case of air-waves; while I have no hesitation in believing that the view thus presented can be so re-enforced by analogous phenomena in Nature all around us, as to render it not only highly probable as the true solution, but almost rationally certain. At all events, I propose now to show that it not only has this reasonable and consistent ground for acceptance as the true explanation of these phenomena, but that it is completely justified and warranted by the voluntary admissions of the very authorities I am reviewing, and in such language that there can be no valid objection urged against its probability, especially by advocates of the current theory of sound.

But supposing, before we advance further, that the current hypothesis is correct as to the first branch of the general assumption that these over-tones are really generated by the segmentation of a string into that many ventral and vibrating sections; and admitting it possible that these subdivisions can all vibrate at one time in connection with the fundamental oscillation of the string, and with as many different rates of periodicity as claimed by the theory:—such a state of facts would be entirely consistent with the corpuscular origin of these eighteen distinct over-tones, since each independent section of the string, having a vibrational number of its own, would generate and radiate a system of substantial sonorous pulses which would pass through the air with a periodicity corresponding to the normal oscillation of its ventral segment, as well as agreeing with the observed pitch of its proper harmonic over-tone. If, therefore, it were possible for a single string, as Professor Helmholtz claims, to subdivide itself up into *eighteen* ventral segments, besides its fundamental swing, and thus generate these eighteen tones by as many corresponding rates of oscillation, I would not have to go a single step further for my explanation of over-tones, based on corpuscular emissions; since these vibrational rates in the string would generate the very substantial pulses with the exact periodicity required by my hypothesis, without any of the absurd "superposition" required by the wave-theory.

With this view, therefore, of the origin of these *eighteen* over-tones, I am only obliged, so far as my hypothesis is concerned, to postulate *one* impossibility—the separate and independent oscillations of *eighteen* ventral segments of the string at one time; while Professor Helmholtz is compelled to assume *three*, by extending these eighteen rates of periodicity to eighteen superimposed systems of air-waves, and then, finally, to eighteen independent rates of tympanic vibration at one and the same time!

The corpuscular hypothesis, therefore, even accepting the *first impossibility* as a basis, steers entirely clear of the *other two*, either of which is infinitely more inconceivable than the first, since we do know, by actual observation, that a string can vibrate in separate ventral segments, to a limited number, at one time; while the superposition of air-waves or of tympanic oscillations, even to the number of *two*, has not only never been observed, but has been proved, in a score of different ways, to be impossible according to every known mechanical law or principle of science. Thus, admitting the truth of the first and lesser impossibility, the corpuscular view of the origin of over-tones becomes at once clear and simple, and confessedly *three times* as consistent and reasonable as the current explanation,—involving, as it does, all three of these impossibilities. Can any logical course of reasoning be more plainly self-evident than this?

But suppose, as I insist, that the self-division of a string into eighteen independent vibrating sections at one time is actually and mechanically impossible; and assuming, then, that the fundamental oscillation of the string does really generate substantial sonorous pulses, as my hypothesis requires, is there anything unreasonable or impracticable in the view here taken that the primary sound-corpuscles thus generated should, by subdivision, radiate a secondary system of pulses, these a third, these a fourth, and so on, as already explained, thus giving rise to the various degrees of over-tones observed? I hold not only that such a result would be entirely possible and reasonable, but I will immediately show that it is clearly justified by the teaching of the very authorities I am now reviewing.

To treat the matter specifically, I maintain that there surely can be no greater difficulty in conceiving the idea that primary sonorous corpuscles, passing off from a sounding body, should *give birth to secondary pulses* of smaller corpuscles possessing a faster or slower rate of emission, thus generating these faint secondary tones either higher or lower, than there is in supposing, as Professor Tyndall distinctly teaches, that the *primary air-waves* sent off from a sounding body, after they have left it and started on their journey, may "*give birth to secondary waves*" which will propagate themselves through the air with an entirely new rate of periodicity, and thus generate these over-tones, resultant tones, &c., having distinctly different degrees of pitch! As strange as it may seem to the reader, this is not only taught in unmistakable language, but it is reiterated in several forms, by this author, as I will now proceed to show. Note the following words:—

"*Vibrations* which produce a *large amount of disturbance give birth to secondary waves which appeal to the ear as resultant tones.*"—*Lectures on Sound*, p. 281.

Thus, a primary air-wave has the power of subdividing itself, and giving birth to other waves of a distinctly different periodic rate! Is not this clear? It might be charged, however, that I misunderstand Professor Tyndall. That he does not say that *air-waves* after being generated "give birth to *secondary waves*," but that "*vibrations . . . give birth*," &c. I assert that I do not misconceive his meaning. These "vibrations" refer to the "oscillations to and fro" of the air-particles constituting such primary sound-waves, and not to the vibratory motion of the sounding body itself, which any one can see by reading the context. As a proof that this is his meaning, the reader is referred to the following, where the same author is explaining the action of the double *siren*:—

"The sound of the *siren* is a highly composite one. By the suddenness and violence of its shocks, not only does it produce *waves* corresponding to the number of its orifices, but the *aerial disturbance breaks up into secondary waves which associate themselves with the primary waves of the instrument.*" —*Lectures on Sound*, p. 291.

This language can not be misunderstood. It is the "primary waves of the instrument," or, in other words, the "aerial disturbance" which "*breaks up into secondary waves*," or which *gives birth to them*. Hence, plainly, if a primary wave can "give birth to secondary waves," which can start off into new vibrational rates, thus generating "resultant tones" of entirely different degrees of pitch, I have an equal right to assume that primary sonorous corpuscles may "break up into" or "give birth to secondary" sonorous corpuscles which will pass off at diverse rates of periodicity, and thus "appeal to the ear as resultant tones" as well as over-tones! If secondary air-waves can be born of primary air-waves, after leaving the instrument, and can then change their *vibrational rates* so as to "appeal to the ear as *resultant tones*," two, three, and four octaves lower than such primaries, then surely sonorous corpuscles constituting the fundamental tone of a string, according to my hypothesis, may give birth to secondary systems of corpuscles constituting over-tones, on the same principle, after they have left the generating instrument, of but one half, one fourth, or one tenth such primary periodicity. Is not this inductive reasoning every way logical and consistent, if there is the least rational foundation for the position of Professor Tyndall?

But here comes in the amusing feature of this great writer's unique assumption that "primary waves" can "*give birth* to secondary waves, which appeal to the ear as *resultant tones*." It is well known to every scientific student that "resultant tones," as already explained, are two, three, and even *four* times *lower* in pitch than the primaries which generate them; and hence their air-waves are correspondingly *longer*, since the wave-length of any tone is exactly proportional to its depth of pitch. Professor Tyndall thus presents us with the startling scientific exhibition of baby-waves at their "birth" *three* and *four* times longer than their *mothers!* But what is such a feat as this for a theory which has no hesitation in giving to a trifling insect more physical and mechanical power than is possessed by all the locomotives in the world combined, making it capable of bending "once in and once out," at the rate of 440 oscillations a second, *two thousand million tons of tympanic membranes by the motion of its legs?* Why, then, should it excite a smile when we are informed that *maternal* air-waves, according to this same theory, can really "*give birth* to secondary waves, which appeal to the ear as *resultant tones*," four times longer than these primary parents? Really, we are only just beginning to get an adequate idea of the prodigious capacity of this enormously underrated theory which has stood unshaken for so many centuries!

The new hypothesis, though postulating a somewhat analogous result, does not involve the nativity of any such absurd aerial or corpuscular monstrosities as just described. It only supposes that the primary sound-corpuscles, as they pass off from a sonorific body, scintillate, or "give birth" to smaller secondary particles of their own sonorous substance, and thus become the parents of lesser pulses, which, radiating in new currents, necessarily produce feebler tones, either higher or lower as the case may be, according to the periodicity of these successive scintillations, or according to the vibrational rate at which they follow each other through the air.

Surely Professor Tyndall, who has no difficulty in believing that primary air-waves may, by subdivision or breaking up, "give birth to secondary waves," thus generating tones of a different pitch, ought not to object to my hypothesis of primary substantial pulses giving birth to secondary pulses of a fainter and fainter type, which will "appeal to the ear" as harmonic over-tones in connection with the fundamental sound of the string.

Every phase of the sound question seems to favor this corpuscular idea as the probably correct solution of such exceedingly faint over-tones, rather than the self-contradictory and preposterous abnormality of primary air-waves subdividing themselves, or breaking up into other waves four times as large as the originals, each of which has a fourfold length of "condensation and rarefaction." The very fact that the so-called harmonics of the violin, made in the usual manner with the bow while gently touching the proper node of the string, are always shrill, and heard among the loudest and most distinct tones of the orchestra, being produced, as they are, by the proper vibrations of the corresponding ventral sections of the string, while the same notes generated as over-tones are so extremely feeble that they are only audible to the finest ear, even by the aid of a resonator when no other fundamental tones are being sounded, would seem clearly to indicate that the latter are not generated at all by the same vibratory motion of the corresponding ventral sections of the string which produces ordinary orchestral harmonics.

Here, then, as now presented, is my main argument, against which, I aver, Professors Tyndall and Helmholtz can make no reply. They are themselves wholly estopped by their own reasoning, since they are compelled to assume at least one class of secondary tones ("resultant") which do not originate in any possible sectional vibration of the string, since they are lower than its fundamental note, and hence can not be accounted for on the principle of "vibrational form"! All the talk of these learned physicists, therefore, about air-waves "exceeding the limits of superposition," and then breaking up into secondary waves which give birth to resultant tones, only goes to help the corpuscular hypothesis of sound, as here maintained. I ask no other admission from these high authorities than the fact that "*resultant tones*" can and do originate in the air after the two generating tones of the chord have left the instrument, to prove that *over-tones* may and necessarily should originate in the same manner, whatever that manner may be, and without the aid of the string's segmental vibration, even if any such vibration were possible.

If primary *air-waves*, I repeat, must necessarily "give birth to secondary waves, which appeal to the ear as *resultant tones*," being the only possible way to produce them, *since the string can possess no vibrational rate slower than its fundamental swing*, then surely there is no acoustical nor mechanical reason, which any physicist can give, why the same primary air-waves may not also break up into or "give birth to secondary waves, which appeal to the ear" as harmonic over-tones! If primary air-waves sent off from a string can, as Professor Tyndall teaches, give birth to baby-waves three and four times longer than themselves, it would manifestly be easier, on the wave-theory, and less strain on the primary maternal waves if they should "give birth to [small] secondary waves, which appeal to the ear" as upper partial tones, only one half to one twelfth as long as their aerial mothers!

If, in plain logic, "resultant tones" do not require "vibrational form" or any equivalent segmental vibration of the string to generate them, but can leap forth out of other waves while passing through the air, what, in the name of acoustics, is the use of "vibrational form" or the oscillation of any ventral sections of a string to give birth to over-tones! It is either all nonsensical superfluity, or else this revelation of Professor Tyndall about primary waves giving birth to enormously long secondary waves, constituting "resultant tones," is scientific latitudinarianism in the superlative degree.

Is it reasonable, therefore, or consistent, to suppose that there could be two distinct and directly opposite plans of generating these secondary sounds,—a part of them being produced by the segmental vibration of the string while the fundamental tone is sounding, and another part without any such sectional vibration of the string at all, but generated on an entirely different principle, after the fundamental tone had left the string and started through the air? Such a supposition is manifestly inadmissible.

But now, after having shown by the order, harmony, and consistency of things, the reasonableness of my position—that all secondary sounds, including *upper partial* as well as *resultant tones*, should have but one mode of origination, and that mode the one substantially admitted by Professor Tyndall—*given birth to in the air after the instrument has done its work*—I here undertake to prove by the same authority that *over-tones*, or *secondary harmonics*, which accompany fundamental tones, also do not originate in the "vibrational form" of the instrument, or by the independent oscillation of its sectional subdivisions at all, *but are generated like resultant tones in the air after the tone leaves the sounding body, by the primary waves, as he claims, subdividing or breaking up into*

harmonics as well as into lower resultant tones. I will first show this by continuing the quotation made a moment since, in which Professor Tyndall teaches that the primary waves issuing from the ***double siren*** break up into secondary waves, which also include these upper partial or harmonic over-tones. The reader will mark the language well, as it drives and clinches the last nail for this over-tone problem:—

"The sound of this siren is a highly composite one. By the suddenness and violence of its shocks not only does it produce waves corresponding to the number of its orifices [its *fundamental tone*], but the *aerial disturbance breaks up into secondary waves which associate themselves with the primary waves of the instrument exactly as the harmonics of a string* or of an open organ-pipe *mix with their fundamental tone.* When the siren sounds, therefore, *it emits, besides the fundamental tone, its octave, its twelfth, its double octave* [its upper partial or over-tones], and so on."—*Lectures on Sound*, p. 291.

Corroborative of this, another passage is equally to the point, in which Professor Tyndall is speaking of air-waves becoming *overgrown*, so to speak, to such extent as to exceed the limits of "superposition," and thus break up into over-tones "*which correspond to the harmonic tones of the vibrating body.*" Here are his words:—

"A *single sounding body* which *disturbs the air beyond the limits of the law of the superposition of vibrations*, also produces *secondary waves* which *correspond to the harmonic tones of the vibrating body.*"—*Lectures on Sound*, p. 282.

Or, as before quoted, "the *aerial disturbance breaks up into secondary waves which associate themselves with the primary waves of the instrument*," and thus "*give birth to secondary waves*" which "*correspond to the harmonic tones of the vibrating body*"! Can anything in science be plainer than this?

It is thus clearly conceded by this authoritative writer that these over-tones caused by the breaking up of the "aerial disturbance" into secondary waves are not produced by the harmonic vibration of the ventral segments of the string at all, since they only "*correspond to* the harmonic tones of the vibrating body," whereas they would be the actual harmonics themselves if made in that way! I therefore ask no other concession from our learned authorities than the foregoing, that these harmonic over-tones, as well as differential tones, are the result of the subdivision of the "aerial disturbance" after it has left the string, and thus can not come directly from the "vibrational form" of the sounding body, as laid down by Professor Helmholtz at the very foundation of his theory of over-tones.

Hence, we arrive at the logical conclusion *that all secondary tones, whether upper partial or resultant, originate in the air, after the sounding body has done its work, by the subdivision and radiation of that which constitutes sound itself!*

It only then remains to determine what actually constitutes sound. Is it simply wave-motion or substantial corpuscles? Professors Tyndall and Helmholtz assume, as their theory requires, that air-waves sent off from the vibrating instrument are all there is involved in its phenomena; and that, by breaking up and subdividing, all these secondary tones are produced. I assert that this assumption has been utterly and disastrously overthrown in numerous ways during the progress of this argument, by showing the impossibility of wave-motion being the cause of sound. Hence, I feel sure the reader must agree with the conclusion that these secondary sounds can not originate by the breaking up of one system of air-waves which have no existence in Nature, and thus give birth to another system equally having no existence, while having, as assumed, an entirely different rate of vibration, and several diverse degrees of amplitude and of wave-length. Hence, the final and only possible conclusion is, that, if substantial sonorous pulses be admissible at all (and the preceding considerations must determine that), the subdivision of their corpuscles into lesser and lesser secondary radiations, having the proper periodicity, must be the only rational solution of all such secondary sounds.

If Professor Tyndall should object to these successive radiations of already infinitesimal sonorous corpuscles as being too "thin" to admit of such subdivision, and as being beyond our comprehension or even conception, I refer him to his own words concerning the corpuscles of *ether*, an hypothetic and all-pervading substance which is so attenuated that 699,000,000,000,000 of its waves a second may dash against the *retina*, as recently quoted, without injury to that sensitive organ! He also says:—

"The intellect knows no difference between *great* and *small:* it is just as easy, as an intellectual act, to conceive of a vibrating *atom* as to conceive of a vibrating *cannon-ball;* and there is no more difficulty in conceiving of this *ether*, as it is called, which *fills space*, than in imagining *all space to be filled with jelly*."—"Within our atmosphere *exists a second and a finer atmosphere* [ether] in which the *atoms of oxygen and nitrogen hang like suspended grains*."—*Heat as a Mode of Motion*, pp. 264, 345.

This is manifestly getting substantial corpuscles down as "thin" as required for my hypothesis of sound, even with its sonorous particles scintillating secondary radiations of smaller and "finer" corpuscles, constituting, as I have assumed, these upper partial and lower resultant tones.

If Professor Tyndall can not understand how such secondary corpuscular radiations can dart off through the air at different rates of periodicity, corresponding to these various over-tones of different pitch, let him explain to the readers of his book how a primary system of air-waves can subdivide itself by *exceeding the limits of superposition*, and thus *give birth to secondary waves*, which propagate themselves through the air at various rates of periodicity, both faster and slower than the primary system, corresponding to all the upper partial as well as lower resultant tones, and I will agree to at once adopt his explanation for the secondary corpuscular radiations involved in my hypothesis. This is surely a fair proposition to the wave-theory.

Having thus endeavored to give my reasons, in general terms as well as in detail, for rejecting the explanation of the cause of over-tones offered by the wave-theory, and in favor of the more simple, consistent, and evolutionary hypothesis of corpuscular emissions as the true solution of the problem,—let us now look for a moment at the beautiful analogical phenomena existing all around us favoring the latter eclaircissement, while we note the unquestionable fact that not one single analogical consideration can be found in Nature (not even *water-waves*, as will be seen in the next chapter) favoring the assumption of physicists that these secondary tones owe their origin to the unparalleled phenomenon of one system of air-waves breaking up and giving birth to other systems, each of an independent periodicity or "vibrational number," and some of them several times larger than their primary parents.

If sound really consists of substantial sonorous pulses instead of the wave-motion of the *medium which conducts it* (which the ultimate overthrow of the current hypothesis must fully establish, as no doubt most physicists would readily admit, since there seems to be no middle ground to assume), there will then be no difficulty in conceiving the fact that the sonorous particles thus constituting a sound-pulse might contain within their substantial elements the principles and radiating forces necessary to generate these secondary emanations. For, if original sound-corpuscles can pass off from a string by some unknown law of radiation and conduction at the rate of one thousand feet a second, there would seem to be no good reason why smaller scintillating particles might not also dart off from these primary corpuscles in various directions by the same law, and from these again others, and so on for each successive over-tone; and, as already observed, far beyond the powers of human observation.

There would appear to be no reason, judging from analogy, why a substantial sound-pulse should not radiate secondary sonorous corpuscles with such variety of periodicity as would constitute tones of different pitch, when the substantial corpuscles of odor passing off from a single flower can radiate atoms, or give birth to secondary fragrant pulses, which appeal to the olfactory nerve as different and distinct perfumes! A certain rose, for example, as my own sense of smell bears me witness, may not only be rich in the prime or fundamental fragrance of its genus *rosa*, but may also radiate at the same time the faint partial smells or odoriferous overtones of both *tea* and *musk* as its upper harmonics. And as that wonderful musical genius, Blind Tom, will instantly name off correctly every note, when a discordant mass of a dozen digitals is struck on a pianoforte at one time, alone by the analytical powers of the auditory nerve, so a certain perfumer in New York is well known to the writer, whose olfactories are so sensitively acute and so educated by practice that he is able to disentangle in an instant an unknown mixture containing half a dozen or more essential oils, or other odorous substances, and name each ingredient, alone by the analytical powers of the nose! The beauty of this analogy existing between the *nose* and the *ear* and between the universally admitted particles of substantial *odor* and what I claim to be the equally substantial corpuscles of *sound*, can hardly fail to impress the mind of the reader with the remarkable similarity in this analytical operation of the two nerves.

It is a well-known fact, worthy of remark, that the analogy existing between the *eye* and the *ear* and between *light* and *sound* is constantly referred to by physicists when treating on the phenomena of hearing and of sonorous propagation; but I have yet to see the first hint or reference, directly or indirectly, in any of their writings, to the manifest and wonderful analogy existing between the *ear* and the *nose*, or between the action of *sound* and that of *odor!* Why this universal and apparently studied omission? There can be but one intelligible reason assigned for such seemingly wilful and concerted ignoring of the most beautiful and startling analogies in Nature, and the utter silence of physicists in regard to their numerous parallel phenomena, and that is this: that any reference to the substantial corpuscles of *odor* and the action of the *nasal membrane* or of the *olfactory nerve* in receiving and transmitting to the brain the sensation of *smell* as analogous to that of *sound* and the action of the *ear*, would instantly overthrow the wave-theory! Who could believe in sound as wave-motion after the admission of any such analogy? But since the comparatively recent origin of the undulatory theory of light, based on the wave-theory of sound, thus making *ether* the analogue of *air* and the *retina* the congener of the *tympanic membrane*, it becomes perfectly safe and scientifically legitimate, in the estimation of these careful and candid investigators, to constantly remind their readers of the remarkable analogy between the *eye* and the *ear*, and the numerous points of resemblance between the action of *light* and that of *sound!*

Judging from all my reading on the subject, and I have read very carefully on this question, it is safe to infer that if *light* were now universally accepted as the emanation of substantial corpuscles, as it was before the time of Sir Isaac Newton, Professors Tyndall and Helmholtz, in advocating the popular atmospheric wave-theory, would be as careful to avoid any reference to the beautiful analogies existing between *light* and *sound* as they now are to give a wide berth to those existing between *sound* and *odor!*

It is anything but agreeable to be compelled to believe such a state of facts, and even more unpleasant to be forced to thus charge home upon the greatest modern investigators of science any such superficial onesidedness; but this monograph would be inexcusably imperfect, and the writer justly chargeable with direliction of duty to the young scientific students of our colleges and other institutions of learning, if this narrow-minded, not to say disingenuous, tendency of our greatest so-called impartial scientific investigators were not laid open to the world as it deserves, and as a warning to future scientists.

There is no question but that an analogy exists between the modes of operation of all the senses, from the lowest or most limited (that of *touch* or *palpation*) up to the highest or most unlimited (that of *sight*); yet not much as between the lowest and the highest, taken at a single step, though the gradation upward is beautiful, and the transition as each step is taken from sense to sense is perfect. In the sense of taction the sensation depends upon the actual contact of the body felt, and not of its radiated or diffused corpuscles, and therefore the distance is nothing.

Taste is greatly similar, yet it borders slightly on smell, since a pungent flavor touching the palate or any portion of the gustatory membrane, instantly diffuses itself throughout the entire mouth, from the lips to the laryngeal region.

Smell, next in the upward order, is higher than taste and approaches hearing, receiving the atoms of perfume at a distance from their source, as they radiate from the odorous body through the air, and with considerable velocity, though much less, and of vastly less range than that of sonorous pulses.

Though *hearing* can reach to a still greater distance than *smell*, yet the difference is almost as nothing contrasted with the immeasurable difference between the range of *sound* and that of *light*.

Although the range of vision and the inconceivable velocity of light almost infinitely surpass those of hearing and of sound respectively, yet there are many beautiful analogies between them, especially those of *reflection* and *convergence;* while there are many marked dissimilarities, such as the absence of shadow in sound, and its power of penetration and conduction through all substances, while light can pass through no opaque body whatever!

There is also a great difference in the analytical capacity of the two senses. The eye can not analyze a single ray of light, and resolve it into its primary colors of the spectrum till it has been separated by the prism; yet the ear is capable of grasping and disentangling the separate notes of a complex chord, as just illustrated in the case of Blind Tom, while the *nose* in a similar manner vastly surpasses the *eye* and almost equals the *ear*, as just shown, by its capacity for separating and recognizing the individual constituents of a conglomerate mixture of different odorous substances.

So natural and unstrained is this manifold analogy existing between *sound* and *odor*, and between *hearing* and *smell*, that among the uneducated masses almost universally a *strong effluvium* of any kind is referred to as a "*loud*" smell! To speak of an intense light as being "*loud*" would be so evidently strained and far-fetched that the intuitive employment of slang among the vulgar has never yet led to its use, though a flash dress of very brilliant colors has sometimes been spoken of as "*loud*." Yet physicists, noted for almost judicial candor and fairness in their investigations of science, as just seen, deliberately ignore these marked analogies between the two senses, which do not reenforce wave-motion, for no visible reason except that they would prove utterly ruinous to a pre-adopted theory. To deny this manifest analogy between sound and odor and between the auditory and olfactory nerves is impossible. To attempt to give any other explanation of the universal silence of physicists on the subject, when writing on sound, is equally out of the question.

The sense of *taste* also possesses an immense register, as well as remarkable analytical powers like those of *smell* and *hearing*,—in this respect also surpassing the *eye*, as it can detect and recognize different degrees of gustatory sensation equivalent in extent to many octaves of sound. In fact, the register of distinct and sensible degrees of *saporosity* which a palate is capable of analyzing and distinguishing, from the lowest notes of *bitter* to the highest tones of *sweet*, not only surpasses that of the eye, even after the ray is separated, but equals that of the nose and very nearly that of the ear.

It is simply surprising when we come to reflect upon the scores of different sensible gradations of the low pungent, bitter, and acrid flavors, alone, which the palate can separately recognize, and then the equal number of degrees of acidity; and, finally, the almost endless varieties embraced in the sweets and fruits of Nature, including the viands, condiments, desserts, and relishes developed by the culinary art.

These analytical powers of the sense of taste are so perfect that a number of different kinds of spice—such as clove, cinnamon, nutmeg, &c., or other highly flavored substances,—may be thoroughly pulverized and mixed, and a pinch of the compound be placed upon the tongue, yet the composite mass can be at once analyzed by the palate, and each individual ingredient definitely determined by a proper effort of attention, the same as the nose can untangle a combination of different odorous substances, or the auditory nerve analyze and separately recognize a composite sound, designating the constituent elements of the chord. Yet who supposes that the gustatory membrane and nerve receive their impressions of taste by the *vibratory motion of the palate* rather than by means of the actual contact of the corpuscles of flavor?

It is also a noticeable fact that flavor can produce a persistent or a kind of *resonant* effect upon the gustatory membrane, which will continue to *ring* even for some minutes after its substantial corpuscles have entirely left the mouth. It is on account of this persistent impression that the intensity of sweet, for example, may be augmented through contrast by previously tasting some sharp acid, such as lime-juice, and *vice versa;*—just as the action of a high or shrill tone on the tympanic membrane causes a low note immediately following to appear lower than it really is, and *vice versa.* Professor Tyndall would, of course, undertake to account for such tympanic effect by insisting that the drum-skin of the ear "can not come instantaneously to rest" after being "once shaken," or thrown into vibratory motion, though he would hardly venture to claim that the palate continues on oscillating after receiving a *sour* impetus from the gastronomical undulations of tartaric acid!

I might easily extend this analogy to *combinational* or *over-tastes*, as the experience of any one with a little attention will confirm. Under civilized improvements in the culinary art we can scarcely taste an article of food which does not contain the upper partial flavors of spices, condiments, seasonings, or relishes of some kind, in addition to the normal flavor of the viand proper, which an effort of attention can easily recognize as the saporific harmonics in the scale of gastronomy,—while, without any additions by the *cuisine*, we all know that the delicate flesh of the snipe or woodcock, if left an hour too long in the sun before being prepared for the table, will so far "exceed the limits of superposition," acting under the law of some sort of gustatory "parallelogram of forces" as to "give birth to secondary waves" of flavor, the "algebraical sum" of which will appeal to the palate of the epicure as a *resultant* taste, producing anything but gastronomic harmony!

It matters very little to me, therefore, if physicists, in their confused and onesided attempts to harmonize the inconsistencies of the wave-theory while treating on *sound* and the mechanism of the *ear*, dare only call attention to the analogies of *light* and the structure of the *eye*, in order to reenforce that hypothesis. The advocate of the evolution of sound, from its low and superficial base of wave-motion to the higher and sublimer level of corpuscular emanations, is not forced into any such asymmetric science by blotting out a part of Nature's analogical chart. He has no need for keeping back a part of the price, or for suppressing a single page of the record of Nature, since he has no such circumscribed and limping hypothesis to maintain. He is not tied to the superficialities of incidental air-waves which sometimes result from sound-generation, but which have no more causal relation to the propagation of tone than the incidental lengthening of the shadow of a tree has to the setting of the sun, or to the revolution of the earth! He sees in this shallow attempt at the solution of sonorous phenomena the same puerility which the far-reaching and evolutionary grasp of Copernicus discovered in the superficial and weak conceptions of philosophers of his time, who persisted in maintaining the Ptolemaic view of the solar system, based on the mere surface appearances of solar and stellar movements. He recognizes, in carefully investigating the phenomena of sound, an intimate and connected correlation linking all the senses into one beautiful and homologous concatenation, from the lowest to the highest; and rationally concludes that if the first three—*touch*, *taste*, and *smell*,—depend for their sensations, as the whole world admits, upon the absolute contact of substantial corpuscles, that it is unwarranted and illogical in the highest degree, unless from overwhelming facts to the contrary, to assume that the remaining two senses —*hearing* and *sight*—should constitute a departure from this inauguration of Nature's plan, and thus abruptly sever its analogical chain.

Is it not every way in harmony with correct ideas of order and congruity of purpose in the working of Nature's processes, that corpuscular contact, which admittedly prevails in the operations of the first three senses, should continue unbroken through the other two, with the corpuscles of sound and light, inconceivably more tenuous, and radiated under the control of subtler and more refined laws, rather than to assume a change in this consistent and beautiful programme by postulating another and unnecessary arrangement utterly unlike that governing the first three, and without the least regard to unity of design or continuity of operation?

In thus assuming to discard the surface ideas of wave-motion and to explain the problem of over-tones, resultant tones, &c., by the hypothesis of secondary radiations of substantial pulses, we are taught to reject mere appearances as generally superficial and false; and this is re-enforced by the fact everywhere observed in Nature, that what appears as a single substance becomes, when analyzed, a duality, and oftentimes a multiplicity of distinct substances, so interblended as to utterly defy the powers of human observation till they are separated.

That primary sonorous corpuscles should contain within their substance the entitative elements and forces which constitute and radiate these faint and almost inaudible over-tones, is no more of a mystery than that a single drop of apparently homogeneous blood should not only be constituted of a multiplicity of separate globules, but that each globule should be a composite and heterogeneous mass, containing not only its primary elements of albumen and fibrine, but also its fainter ingredients of iron, salt, lime, sulphur, sugar, phosphorus, magnesia, and even water, whose separate corpuscles are also composite and constituted of independent atoms of oxygen and hydrogen! No more marvelous than that the golden nuggets cast forth from the secret laboratory of Nature should contain, besides the prime metal, the "harmonic over-tones" of silver and copper; or even the faint "partial notes" of nickel, bismuth, or other metalliferous substances. No more wonderful, carrying the mystery from the physiologic, metallurgic, and acoustical world, into the realms of psychology, than that the fundamental passion of *love* should contain within its elemental nature the substantial "over-tones" of *jealousy*, *hope*, and *fear*, blended many times with the apparently antagonistic but deeply rumbling "resultant" or "differential" notes of *anger*, *hate*, and *revenge!*

In this analogous manner, as just seen, a single sensation of taste may recognize the presence of half a dozen distinct flavors,—a single sniff of odor may convey to the analytical department of the brain adapted to this sensation a number of separately recognizable grades of perfume,—while a single fundamental sound can be analyzed by the auditory apparatus exactly in the same way, and may thus be found to contain several distinct over-tones of different degrees of pitch and intensity.

Thus each of the senses, including the substantial corpuscles actuating it, has its *range* as well as its *register*,—while every sensation is equally the result of absolute corpuscular contact with the appropriate sense-membrane. Without this there is no consistency nor analogical harmony in the plan of Nature. For example, as the auditory nerve recognizes the octaves of sound by their *pitch*, from the slowly pulsating bass to the rapidly throbbing soprano, so the optic nerve recognizes its single octave of light in its variety of *color*, from the deep notes of vermillion and crimson, through the middle register of green and yellow, up to the highest tints of blue and violet: and as the gustatory nerve recognizes its octaves of taste by variety of *flavor*, from the low and shuddering notes of aloes and wormwood, through the mean register of acids, up to the purest and highest tones of nectarous sweets,—so the olfactory nerve recognizes and analyzes its numerous octaves of odor by their variety of *scent*, from the low-exhalations of putrid substances and the repugnant effluvium of the sty, up through the numberless gradations of agreeable perfumes, finally culminating in its highest octave, containing the exquisite fragrance of the rose and the pink, the ineffable and delicate sweetness of the hyacinth and honeysuckle, and the matchless richness of the heliotrope and lily of the valley, which may be accented as among the purest harmonics of this wonderful odoriferous scale!

Analogies like these existing between the different senses, particularly between those of *taste*, *smell*, and *hearing*, and consequently between *flavor*, *odor*, and *sound*, with two of them the acknowledged results of corpuscular contact, could hardly be supposed to exist unless the other was equally the result of analogous substantial pulses! While physicists would never think of calling to their aid any kind of wave-motion, either of the *air* or *ether*, in accounting for the sensation of *taste* or *smell*, and would resort to no oscillatory movement whatever, either of the *palate* or of the *nasal membrane*, in order to explain the wonderful analytical powers of these organs in disentangling the most complicated mixtures of *flavors* and *odors*, is it reasonable, I again ask, that they should upset this consistent programme as soon as they come to *sound*, and thus violate the unity and continuity of Nature's plan by making the sensations of tone depend upon the manifestly impracticable wave-motion of the air, the impossible oscillation of the tympanic membrane, or the ridiculous "unisonant vibration" of Corti's microscopical rods?

And lastly, we have in perfume the startling analogue of *differential* or *resultant tones* by the mingling of a *chord* of two distinct odors, and thus generating a third effluvium essentially different from either.

It is well known to chemists that if a solution of ammonia is saturated with sulphureted hydrogen gas, each possessing its own peculiar and characteristic odor, a compound is obtained called sulphide of ammonium. In this compound an experienced observer can easily detect three distinct smells, namely, that of ammonia proper, that of sulphureted hydrogen proper, and besides these a *resultant* or "*differential*" smell entirely distinct from either, which clearly results from the combination. There is no "*vibrational form*" about this resultant smell which produces the peculiar "quality" of the odor, while physicists will hardly undertake its solution by the "superposition" of a number of systems of odoriferous undulations, aided by the "parallelogram of forces," thus making up the "algebraical sum" of all the different systems of smell considered individually, as does Professor Helmholtz in accounting for combinational tones!

In conclusion, I will only repeat in substance what I have before intimated, and now wish to impress upon the mind of the reader, that if the sensitive membrane of the nose is capable of receiving and transferring to the olfactory nerve the numberless varieties and shades of perfume of which Nature is so prolific, each one of which is separately conveyed to the brain and there translated into its proper individual sensation, without the aid of any vibratory motion whatever of this membrane, and without the dashing of superimposed waves of *air*, *ether*, or any other kind of substance save that of the granules of odor itself, and with the whole scientific world admitting perfume to be a substantial emanation of corpuscles, though unrecognizable by any other of the senses,—is it not reasonable and every way consistent to assume, as I have done, that sound likewise is an emanation of substantial corpuscles, also unrecognizable save by a single sense; and is it not rationally probable that such sonorous particles act on the sensitive membrane of the ear, and through it on the auditory nerve, and finally on the brain, in substantially the same manner as do the corpuscles of odor, without the intervention of air-waves or any vibratory motion of the ear or its individual parts, especially in view of the various classes of facts and arguments brought to bear in this chapter against the current theory of sound?

I therefore, with the utmost confidence in its truth, submit the new hypothesis (with my reasons in part for rejecting the old one) to the unbiassed judgment of physicists, especially such as are not directly and personally committed to the wave-theory of sound, confidently expecting that a verdict will be rendered alone in the interests of science.

CHAPTER VI.

EVOLUTION OF SOUND.—Review, &c., Continued.

A New Class of Arguments Introduced.—The Impossibility of Wave-Motion in Solids, such as Rock, Iron, &c., demonstrated.—"Condensations and Rarefactions," the only Sound-Waves claimed by Physicists, an Absurdity when applied to Rock or Iron.—The Similarity of Water-Waves and Sound-Waves admitted by Physicists.—This Fact alone Fatal to the Wave-Theory.—Many Reasons given for it.—The Uniform Ratio of Amplitude to Wave-Length about 1 to 10 in all True Waves.—Absence of Amplitude in Iron Sound-Waves demonstrated, while Certain Waves are Proved to be 476 feet long.—Infinite Difficulties in the Way of the Theory.—The Absence of Amplitude confirms the Corpuscular View that Sound passes in Straight Lines.—Fatal Admissions by Professors Tyndall and Helmholtz.—A Condensed Account of an Interesting Investigation of the Wave-Theory with a Scientific Friend.—Numerous Objections Raised and Answered.—The Wind Proved to have no Effect on Sound.—The Evidence of the Signal-Service.—A Strong Argument against the Wave-Theory, and in Favor of Corpuscular Emanations.—Professor Tyndall's Illustrations of a Row of Boys and a Row of Glass Balls Exploded.—Physicists shown to be Dishonest without intending it.—Professor Tyndall's Illustration of the Tin Tube and the Lighted Candle Annihilated.—His Illustration of the Resonant Glass Jar and the Quarter Wave-Length Hypothesis Scathingly Reviewed.—Another Illustration, showing that sounding two Forks half a Wave-Length apart will produce Interference, Reviewed and Exposed.—No Foundation in Truth for the Assumption.—The Explanation of the Interference of the Double Siren, as given by Physicists, Explained Away.—No Interference about it.—A Serious and Fatal Misapprehension.—An Unmistakable Test Proposed to Professor Helmholtz by which to Determine the Whole Question.—The Wave-Theory Self-Contradictory and Self-Neutralizing.—Musical Beats Explained Scientifically.—Their Production by Interfering Air-Waves Shown to be Impossible.—The Konig Instrument for Dividing a Stream of Sound into Two Branches Explained.—Professor Tyndall's Statements Positively Denied.—His Contradictions, Inconsistencies, and Numerous Scientific Errors Pointed Out.—A Final Overwhelming Argument based on the Nature of Wave-Motion which Alone Breaks Down the Current Theory.—Note on the Supposed Sympathetic Vibration of the Antennæ of the Mosquito.—An Amusing Exposition of Professor Mayer's Hypothesis.—Addenda to Chapter VI.

In concluding this examination of the Undulatory Theory of Sound, it is my purpose to devote the present chapter to an entirely new class of arguments bearing directly against the hypothesis. Although it might be considered almost a work of supererogation to the reader who has attentively followed the argument through the preceding chapter, yet the overthrow of the theory may not be considered complete so long as physicists can point to a single consideration appearing to support the hypothesis which has not passed under review. I shall, therefore, not only undertake to introduce a number of new and overwhelming arguments against the current theory, but shall call the reader's attention especially to the enormous and glaring impossibilities to which physicists are compelled to resort in order to sustain the idea of wave-motion and make it appear feasible. If, therefore, in these animadversions, it shall become necessary to expose to an unenviable view the hollow scientific pretensions of some of our greatest authorities on sound, no personal construction must be placed upon language which is only intended to apply to the theory itself and to the arguments employed to sustain it.

With these preliminary remarks I come directly to the question in hand, and will in the first place look at what I conceive to be one of the most manifest and self-evident impracticabilities of the wave-hypothesis viewed from a common stand-point, and based upon the universally admitted facts and figures of the theory, about which there can be no dispute among writers on acoustical phenomena.

That sound passes through *wood, water, rock, iron*, and other solid and fluid substances, no one questions; and that it passes through these substances on the same principle and according to the same uniform laws of propagation as through *air* I shall assume as granted, or at least incontrovertible, from the very necessities of the case, since such a thing as two modes of sonorous propagation was never intimated by any writer on the subject, ancient or modern. To assume two modes of conduction through any two substances—one wave-motion and the other something else—would be to at once open the floodgates of logic, and make a separate and dissimilar mode of propagation possible or even necessary through every known substance, from hydrogen gas to platinum. There is therefore no view admissible or supposable except the one here assumed, namely, that sound travels through all bodies, of whatever density or rarity, gravity or levity, on the same uniform principle and by the same established law of conduction and radiation as it passes through *air*.

Should it, therefore, now be demonstrated that sound does not and can not travel through rock, iron, water, or other solid and liquid substances, by the wave-motion of such conducting mediums, or the oscillation "to and fro" of their particles, a child must see that it can not travel by wave-motion through air, and hence that the whole undulatory theory falls to the ground. The sequential correctness and necessity of this conclusion are unquestionable.

Let us approach this impracticable feature of the theory gradually and with careful deliberateness. First, I would seriously ask the reader if he believes it possible that the scratch of a grasshopper's feet or the chirruping of a cricket upon one end of a long pine tree is capable of throwing the entire mass of wood into undulations? He must believe it if he is ready to subscribe to the wave-theory, since such a sound can be distinctly heard at the other end of the trunk, three hundred feet distant, if the ear is placed properly against it!

Would not the common sense of any unbiassed thinker revolt at the supposition that all the molecules constituting that mass of wood were actually caused to oscillate *"to and fro with the motions of pendulums,"* which are the words employed by Professor Mayer, as well as by Professor Tyndall, in reference to the action of sound-waves in air? I use the phrase "common sense," for the reason that every one possesses more or less of that commodity who pretends to think at all. It does not require extensive scientific culture to grapple with this question. It is one of the simplest problems in the whole range of mechanics. No physical effect can be produced without an adequate corporeal cause; and in mechanics the common sense of a child assures him that an insect with scarcely appreciable physical strength could not stir such a mass of ponderable wood at all, or the hundred thousandth part of it, let alone throwing its entire substance into undulations by which each atom must make a separate "small excursion to and fro," and keep up these excursions at the rate of several hundreds a second!

Hence, this single fact that a sound produced by such a trifling mechanical force as the movement of an insect's feet, will permeate and pass through the entire substance of such a mass of wood, weighing several tons, is demonstrative proof, as strong as proof can be, that it is not done and can not be effected by the wave-motion of the tree, either internally or externally, or the displacement of its material particles, causing them to oscillate "to and fro with the motions of pendulums" several hundred times a second, which must obviously be the case if there is any truth in the wave-theory.

These remarks also apply equally and with even greater effect to the passage of sound through *rock* and *iron*, since they are denser, and must necessarily require greater mechanical power to throw their molecules into oscillatory motion; yet the scratch of a pin on one side of the Rock of Gibraltar could be heard through it by placing the ear against the opposite side, aided by a stethoscope. I aver that no well-balanced mind can believe, when it comes seriously to reflect, that a large mass of rock or iron through which such a sound passes is actually thrown into vibratory motion, and its separate particles made to oscillate "to and fro," as air-particles are supposed to oscillate by means of sound-waves. If not, then the particles of air do not so oscillate, or assume the character of waves, as the cause of sound, and hence the wave-theory breaks down.

Physicists have noticed the fact, when sound passes through a solid body, such as a mass of wood, from a vibrating instrument held against it, that such conducting body experiences a tremor corresponding to the vibrational rate of the sounding instrument, and this circumstance has led them superficially to infer that the tremor of the wood thus produced is the real cause of the sound. I have pointed out the superficiality of these childish observations in numerous places in the preceding chapter. If the vibrating instrument has sufficient *vis viva* while producing the tone to shake the conducting medium with which it is in contact, only for a limited distance around, such effects of course occur *incidentally*, and are, as already shown, no part of the sound produced, neither of its cause, any more than the incidental tremor of the air or recoil of the cannon when discharged is an essential part of the process which hurls the projectile.

These surface observations of sound investigators are unfortunately the very foundation on which the entire wave-theory of sound rests. There is not a physicist who notices the jarring of a membrane at a distance from a sounding body but will instantly jump at the conclusion that the entire body of air between the membrane and the source of the sound must necessarily take on the same vibratory motion! It seems impossible for them to grasp the simple thought that the substantial unisonant sound-pulse itself possesses an actual sympathy for the membrane tensioned to the same vibrational number of the sonorific instrument. They can not see how it is possible for such substantial sonorous corpuscles to dart off from the sounding body to the membrane with such periodicity as to act sympathetically on its unisonant quality and set it to oscillating, unless the entire mass of intervening air takes on a similar oscillatory motion.

It is this very superficial error, so thoroughly ventilated in the preceding chapter, on which the whole wave-theory rests. Yet these very physicists can look on a magnet and see it moving a magnetic needle at a distance and causing it to oscillate and quiver through plates of solid glass, without the remotest idea that such effect is produced by any disturbance communicated to the intervening air! They even do not hesitate to concede that substantial but intangible corpuscles of some kind may radiate from the magnet to the needle, passing unimpeded through the glass, and thus mechanically move the needle. Yet they can not conceive of sonorous corpuscles radiating in synchronous pulses and in a somewhat analogous manner, acting in periodicity to a unison membrane, thus causing it to vibrate, without a corresponding motion of the *intervening air*.

One would really think that a physicist who had ever seen a steel magnet, and noted its action on a compass-needle through plates of impervious glass, would have found sufficient cause for at least suspecting the wave-theory of sound, if not for repudiating utterly the unspeakable impossibility of an insect shaking four square miles of atmosphere, and of exerting, by the simple movement of its feet, *millions of tons of mechanical force*, as demonstrably shown in the preceding chapter. (See pp. 133, 134, &c.)

We shall try to show the reader in this chapter, if it has not already been sufficiently done, the scientific distinction which must be borne in mind between *sound* as the primary result of instrumental vibration and those incidental effects of tremor produced upon the conducting medium near the instrument by the same motion which generates the tone.

Another preliminary proof that sound can not and does not pass through a mass of solid rock or iron by means of wave-motion is deduced from the essential definition of a sound-wave as given by physicists. Water-waves, which are referred to by all writers on sound as illustrative of air-waves, have room to rise and project the water above its surface-level in the form of ridges which necessarily leave corresponding depressions in its surface in the form of sinuses or troughs. But in the midst of the aerial ocean there is no atmospheric surface above which an air-wave can project itself in the form of a crest; hence the wave-theory teaches, as the only alternative, that the air must be *condensed* or *packed* into more closely compressed ridges to represent the crests of a system of water-waves, and be *rarefied* or expanded to represent the furrows, thus amounting to exactly the same thing. Professors Tyndall, Mayer, and Helmholtz, as fully quoted in the preceding chapter, have repeatedly told us that the only kind of a wave which sound can produce in the air is "a *condensation* and its associated *rarefaction*," representing the crest and furrow of a water-wave. "A *condensation* and a *rarefaction*, then," says Professor Tyndall, "*are the two constituents of a wave of sound.*" (See pages 125, 126.)

Now, as "a sonorous wave" in a mass of air, as Professor Mayer expresses it, "is *always* formed of two parts, *one half of air in a state of condensation, the other half of rarefied air*," then it follows, and Professor Mayer can not and will not deny it, that a sound-wave passing through a mass of *iron* must also be formed of "*two parts, one half of iron in a state of condensation, and the other half of rarefied iron*"; that is, according to this highly "scientific" theory, the molecules of iron or rock throughout the entire mass permeated by the sound must be alternately *compressed or squeezed more closely together*, and then *expanded more widely apart* several hundred or perhaps several thousand times a second, according to the pitch of the tone.

Is the reader prepared to accept this essential and indisputable feature of the wave-theory of sound, namely, that the stridulation of a locust, for example, sitting on a rock, actually throws the molecules of the entire mass of granite first into *condensations* and then into *rarefactions*,—first squeezes the particles of stone more closely together, and then rarefies or expands them more widely apart? If he does not and can not believe this, then he does not and can not believe that sound passes through rock or iron by wave-motion at all, and hence that wave-motion is also out of the question in *air*, as this is the only possible form of a wave which can occur in the interior of a mass of any kind of substance such as *air*, *water*, *wood*, or *iron*, as distinctly taught by Professor Helmholtz and all writers on sound.

As such a preposterous result as the compression of the particles of a granite rock by the physical strength of an insect is revolting to every idea of mechanics, and overthrows all known relations existing between cause and effect, it follows that the idea of sound traveling through rock or iron by wave-motion must be a manifest scientific fallacy, and hence that wave-motion in air equally falls to the ground, since in the very nature of things, as before shown, there can be no two modes of sound-propagation through different substances.

It need not be said here that the sound of an insect would not permeate a rock. Why, the pulverizing of a granite rock a hundred feet square to powder would be almost as nothing to the task absolutely performed by a locust, according to the wave-theory, in converting four cubic miles of atmosphere into "condensations and rarefactions," exerting sufficient pressure and thus generating sufficient heat to add one sixth to the velocity of sound throughout this entire mass of air! (See pp. 145, 146.) The most trifling sound produced against a mass of rock ten feet thick, even the movement of an insect's feet, can be heard through it, as just remarked, by the aid of a stethoscope. According to the wave-theory this is only effected by the particles of stone being thrown into undulations, consisting of absolute "condensations and rarefactions."

But further, in the preparatory discussion of this argument, we are taught by Professor Tyndall and Laplace, as just intimated, that the *squeezing* of the *air-particles* together generates heat (as it necessarily must do), which adds *one sixth* to the velocity of sound in air; and hence it follows, as the same "condensations and rarefactions" must take place in a mass of iron, since there must be the same wave-motion and almost infinitely greater compression exerted, that they also must generate heat at each compression or condensation of the iron-particles, which should also augment the velocity of sound through all such solid substances in like proportion. But as iron once heated to any degree whatever can not instantly become cool, even if dipped into cold water, it would therefore be impossible for any one of the 440 condensations a second, produced by the stridulation of the locust, to cool off by its associated rarefaction before another condensation with the same heat would re-enforce it. Thus, the heat generated by one condensation of the iron could not have time to subside in any calculable degree before its re-enforcement by another, that by another, and so on, at the rate of 440 a second, if the pitch of the stridulation should be that of A, or the same as that of the second string of the violin. It is thus perfectly manifest, according to the wave-theory, that a locust by singing for one minute, sitting on a mass of iron, ought to raise its temperature to incandescence; for however little heat a single "condensation" would produce, this rapid accumulation, without time for subsidence, would necessarily accomplish this miraculous result. But as not the slightest heat is generated by the passage of sound through iron or any other solid body, I care not how intense or how long continued such sound may be, it follows that no "condensation" and hence no wave-motion can take place in the passage of sound through any substance whatever!

All writers on sound tell us that the material particles of any body constituting the sonorous wave, though they do not travel forward with the undulation or swell, yet have a "to and fro" movement, once up and once down as each wave passes, as observed in the up and down movement of a chip floating on the surface of water disturbed by waves. Any one knows that without this there can be no such thing as wave-motion. This same "to and frô" movement of the air-particles is claimed to take place in the passage of a sound-wave by both Professors Helmholtz and Tyndall, and in fact by every authority on sound. I will quote a few sentences from these writers to make clear this principle, so the reader will not have to take my bare word for anything. Professor Helmholtz, in speaking of waves caused by throwing a stone into water, remarks:—

> "The waves of water, therefore, continually advance without returning. But we must not suppose that the particles of water of which the waves are composed advance in a similar manner to the waves themselves. The motion of the particles of water on the surface can easily be rendered visible by floating a chip of wood upon it. This will perfectly share the motion of the adjacent particles. . . . By these examples the reader will be able to form a mental image of the *kind of motion* to which sound belongs, where the *material particles of the body merely make periodical oscillations*, while the tremor itself is constantly propagated forwards. . . . The process which goes on in the atmospheric ocean about us, is of a precisely similar nature. For the stone substitute a *sounding body* which *shakes the air;* for the chip of wood substitute the *human ear*, on which impinge the *waves of air* excited by the shock, *setting its movable parts into vibration.* The *waves of air* proceeding from a sounding body transport the tremor to the human ear exactly in the same way as the water transports the tremor produced by the stone to the floating chip."—*Sensations of Tone*, pp. 14, 15.

Professor Tyndall says:—

> "The motion of the sonorous wave must not be confounded with the *motion of the particles* which at any moment form the wave. During the passage of the wave *every particle concerned in its transmission makes only a small excursion to and fro.* The *length* of this excursion is called the *amplitude* of the vibration."—*Lectures on Sound*, p. 44.

This is the universal teaching of the wave-theory of sound, namely, that the particles of the medium which conducts the sound make an "excursion to and fro" every time a sonorous wave passes, and that the length of the "excursion" of these physical particles constitutes the "amplitude of the vibration," which is the same as the distance in a water-wave from the top of the crest to the bottom of the sinus or trough.

Thus the materials accumulate in our hands by which to annihilate the wave-theory, if we only apply them properly to the question under discussion. Here we have it, in plain words, that a sound passing through *iron* or any other substance whatever, or, to use the exact words, "during the passage of a wave ☞ *every particle concerned in its transmission makes only a small excursion to and fro*," and that "the *length of this excursion* is called the *amplitude of the vibration.*" This eminent writer will not pretend to say that this does not apply to *iron* as well as to *air*. He would not so stultify logic or insult reason. To attempt such a specious and wretched quibble to escape the consequences of wave-motion would be to make the advocate as ridiculous as the theory will soon be shown to be.

Now, are we able to arrive at a correct

and scientific idea as to this question of "amplitude," or to determine definitely the "*length of this excursion*" which the separate "particles" of *iron* must make "to and fro" in order to constitute a wave proper while a sound is passing through its mass? I assert that we have a definite and positive *law*, laid down by these writers themselves, which is as simple and as impossible to be misunderstood as any question in common arithmetic, telling us just how far these particles of *iron* or *air* must oscillate "to and fro" to constitute this "amplitude," which the reader can not fail to see and also to be astonished at in a moment.

We now come directly to a class of facts which no physicist will pretend to dispute. The only visible wave-motion of which we have any definite knowledge is that which takes place upon the surface of water or other liquid. Air-waves are invisible; and therefore, if they occur at all, as assumed by the wave-theory of sound, we can only understand their form, motion, velocity, &c., and their relation of *amplitude* to *wave-length* by reference to the form and motion of water-waves. Hence it is that physicists (without realizing the ruinous result to their theory) constantly refer us to the undulations produced on the surface of water as *exactly similar* to sound-waves produced in the air, and hence also in any other substance.

I do not exaggerate by saying *exactly similar*, but mean what the words literally imply. As this is essential to my argument, which I mean shall be so fortified at this particular point as to admit of no answer, I will now prove by Professor Helmholtz—the highest living authority on physical science—that sound-waves in air and water-waves are "*essentially identical*," of a "*precisely similar nature*," and travel "*exactly in the same way*"! Here is the evidence, a part of which has just been quoted:—

"Suppose a stone to be thrown into a piece of calm water. Round the spot struck there forms a little ring of wave, which, advancing equally in all directions, expands to a constantly increasing circle. *Corresponding* to this ring of wave *sound also proceeds in the air* from the excited point, and advances in all directions as far as the limits of the mass of air extend. The *process in the air is essentially identical* with that on the *surface of water*. . . . The *process* which goes on in the *atmospheric* ocean about us is of a *precisely similar nature*. . . . The *waves of air* . . . transport the tremor to the human ear *exactly in the same way*."—*Sensations of Tone*, pp. 14, 15.

Many passages from Professor Tyndall's works could be quoted "essentially identical" if not "precisely similar," all bearing on the subject "exactly in the same way"! But these are sufficient, and as explicit as could be desired.

Then what is the *law* revealed by water-waves, according to this emphatic language, as to the question of "amplitude" or "this length of excursion to and fro" of the particles of water constituting the undulation? It is this, and these learned authorities are particularly and earnestly invited to note the crushing fact, that in water-waves, whether large or small, the proportionate relation of *amplitude* to *wave-length* in feet, inches, or fractions thereof, is always about as 1 to 10 or 12, reducing this proportion slightly as the waves increase in size! That is to say, the smallest measurable system of waves, caused by drops falling on the surface of water, has a wave-length or distance from crest to crest of about *one inch*, with an amplitude or depth from crest to sinus of about a *twelfth of an inch*. Waves caused by throwing stones of about a pound weight into water have an amplitude of about two inches, and hence travel about twenty inches to two feet apart, as measured from wave to wave.

I have spent much time in observing and measuring waves of different sizes and generated in various ways, and find this law to be very nearly uniform in its application. Waves when running freely a foot high, after being produced by a passing steamboat, are invariably about ten feet from crest to crest,—while ocean billows, produced by a steady current of wind, if of an average amplitude of about five feet, may fall somewhat short of this average wave-length, being from forty to forty-five feet from crest to crest. Larger billows experience about a proportionate decrease in wave-length in relation to amplitude. Yet the law holds inviolate that the longer the waves from crest to crest the greater must be the amplitude from crest to sinus. There can, in the nature of things, be no exception to this rule.

The very nature of wave-motion precludes the possibility of this law being otherwise, since manifestly a system of ocean billows five feet high could not by any possibility run within a foot of each other, or with only a foot from crest to crest, as it would make their walls so nearly perpendicular that they would break over and blend into each other, thus reducing their amplitude to conformity with the law I have been illustrating. To prevent this breaking over of the wave-crests upon each other it is absolutely essential, as any one can see, that their distance apart must sustain such a proportionate relation to their amplitude or height as will give the sides of their walls the proper inclination or slant to prevent tumbling! Nothing can be plainer to a mechanical mind. Hence, this law of which I have spoken exists in the nature and necessity of wave-motion, and must hold good in waves of air or iron produced by sound, if they occur at all, as well as of water, since they are, as our great German authority teaches, "precisely similar" and "essentially identical."

It is partly this fact which causes the constant display of breakers on a beach. The front waves are retarded by the sand as soon as the water begins to get shallow, thus allowing those behind to approach so near as to vitiate this proportionate relation between wave-length and amplitude, making the walls too steep to support the crests in their symmetrical form, and the result is we see billows continually breaking over into foam on reaching shallow water. This result is also partly due, no doubt, to the fact that the lower portion of the wave being retarded by the sand allows the crest to outstrip the base, which adds to its perpendicularity and augments the tendency to break.

In like manner it would be equally impossible for a system of water-waves, produced by a single exciting cause, to run fifty feet from crest to crest while but an *inch* in amplitude! Such a system of waves was never seen except in the visions of physicists while dreaming possibly about the practical anomalies of the wave-theory of sound.

I have thus reached the culmination of this argument. If sound-waves and water-waves, as we are authoritatively assured, are "*essentially identical*," of a "*precisely similar nature*," and travel "*exactly in the same way*," then this law of proportion in feet and inches between ***amplitude*** and ***wave-length*** must hold inviolate in sound-waves as well as in water-waves, or otherwise they are "essentially" ***opposite***, "precisely" ***dissimilar***, and travel "exactly" in *a different way!*

It now only remains, in order to complete this annihilating argument, to find out if there is such a thing as a definite, measurable *wave-length*, in feet and inches, taught by the current theory of sound, for each determinate pitch of tone. If such be the fact, and each determinate pitch of tone has a definite, measurable wave-length, in feet and inches, then we know, as a matter of course, what must be the *amplitude* of such a system of waves, or the distance the wave-particles have to oscillate "to and fro." There is no possible escape for physicists from this ratio, if sound travels by waves at all. If, for example, the wave-length of a certain tone should be ascertained to be ten feet, we know its amplitude must be about *one foot*, or about one tenth its length, for such we have found to be the infallible law governing water-waves, which are "essentially identical" and "precisely similar," and the only visible criterion we have for determining the mechanical nature of wave-motion! The catastrophe of the wave-theory thus gradually approaches.

I now state, what is well known to every tyro in science, that the wave-theory of sound necessarily teaches that every pitch of tone, throughout the entire range of the musical scale, has a different and determinate wave-length in feet and inches, which is distinctly inculcated by all writers on sound. I do not ask the reader to take my word for this important and pivotal fact in this argument. Here is the explicit evidence from Professor Tyndall:—

"Having determined the rapidity of vibration, the *length of the corresponding sonorous wave* is found with the utmost facility. Imagine this tuning-fork vibrating in free air. [The fork he refers to has 384 vibrations to the second.] At the end of a second from the time it commenced its vibrations, the foremost wave would have reached a distance of 1090 feet in air of the freezing temperature. In the air of this room, which has a temperature of about 15 degrees centigrade, it would reach a distance of about 1120 feet in a second. In this distance, therefore, are embraced 384 sonorous waves. Dividing, therefore, 1120 feet by 384 *we find the length of each wave to be nearly three feet.*" [Exactly 2 feet and 11 inches.]

"A series of tuning-forks stands before you, whose rates of vibration have already been determined by the siren. This one, you will remember, vibrates 256 times in a second, *the length of the sonorous wave which it produces being, therefore, 4 feet 4 inches.*"—*Lectures on Sound*, pp. 69, 172.

Thus we have the definite proof that a tone having 384 vibrations, or propagating that many waves in a second, has an actual wave-length of 2 feet and 11 inches; and if another pitch of tone happens to be composed of 256 waves in a second, its wave-length is literally "4 feet 4 inches" "from condensation to condensation," or from crest to crest.

Now, suppose I should ask Professor Tyndall to tell me the exact or even approximate *amplitude* of the vibrating air-particles in feet or inches for this system of waves which he has here shown to have a determinate wave-length of "4 feet 4 inches,"—could he do it? I answer, emphatically, he could not, and, if he could, he would not dare to; for it is a notorious fact that though these writers on sound are constantly calculating and recording the "wave-length," in literal "*feet*" and "*inches*," of tones of various degrees of pitch, *they have never once, in all their writings, so much as intimated even the approximate amplitude or width of swing of the air-particles in any single system of sound-waves!* The reason for this strange neglect is plain on its very face, of which the reader will soon be entirely satisfied. To name any definite amplitude, or to fix upon any determinate distance which the particles constituting a sound-wave must oscillate "to and fro" would be to at once annihilate the wave-theory if the same amplitude should be applied to a wave passing through a mass of rock or iron, or any other substance whose motion, if it has any, can be seen! Hence, writers on sound invariably speak of this "amplitude" or "excursion to and fro" in a vague and indefinite way, sometimes intimating that if one sound is twice as loud as another it is because the air-particles constituting the wave have twice the "width of swing" in the one case as in the other; then again, when vexed with the problem of "superposition," this "excursion to and fro" becomes "infinitesimal"! I have searched in vain through every work on sound within my reach, to find one single instance where physicists dare come out and say, as any scientific investigator ought to say if he has a consistent theory to defend, how many inches or what fraction of an inch the air-particles travel "to and fro" for any given pitch or any degree of intensity. Should they venture to commit themselves on this subject, the reader must see that such a statement, but once recorded, would write the obituary of the wave-hypothesis.

The nearest to it I have been able to find is the language of Professor Helmholtz in speaking of tympanic vibration, as follows:—

"In this transference of the *vibrations of the air* into the labyrinth, it is to be observed that though the *particles of the air* themselves have *comparatively a large amplitude of vibration*, yet their density is so small that they have no very great moment of inertia."—*Sensations of Tone*, p. 199.

But suppose I should ask Professor Helmholtz what he means, in *inches* or the fraction thereof, by "*comparatively a large amplitude of vibration*," he would be as dumb as death! Though he had explicitly and repeatedly recorded what a "wave-length" is in feet and inches for every pitch of tone, and though he had taught that air-waves and water-waves are "essentially identical," "precisely similar," and travel "exactly in the same way,"—and though an investigator with a thousandth part of his intelligence could not help knowing that a system of water-waves with an ascertained wave-length of "4 feet 4 inches" must have an amplitude of at least 5 inches, in the very necessities of wave-motion, with every particle constituting the waves oscillating to and fro that distance,—yet neither he nor Professor Tyndall ventures an application of this consistent and universal law to these hypothetic sound-waves in air, because, as before intimated (whether they thought of it or not), it would instantly overthrow the wave-theory of sound if the same rule should be applied to iron, wood, water, or any other substance whose particles could be seen, and thus ocularly be demonstrated not to move at all!

In order to utterly expose the absurdity of the theory of sound-waves in *iron*, and hence in any other substance, including *air*, we have only to suppose that the particles of iron constituting a wave move only the hundredth part of an inch "to and fro with the motions of pendulums," and it is easy to see that a mass of the hardest steel, permeated by a sound constituted of several hundred waves in a second, would be pulverized to impalpable dust in less than a minute under such a grinding process. This is the reason, in a nutshell, why it would not do for "science" to specify any definite amplitude for the air-particles to oscillate to and fro, or even to utter one syllable on this subject of the proportionate relation of amplitude to wave-length, which so unavoidably prevails in water-waves, and without which they have no existence!

These profound scientific investigators know very well that the only actual wave-motion which can be seen and measured, and which they declare to be "precisely similar" to sound-waves, is governed by an unvarying law of proportion, just as I have stated it to be, and that waves of water could not exist at all unless this ratio of about 1 to 10 were maintained between the amplitude or width of swing of the wave-particles and the measurable wave-length from crest to crest. Yet knowing all this, as they must, if they possess intelligence qualifying them to write on any scientific subject, and telling their readers at the same time, as they do, that such water-waves are "essentially identical" with sound-waves, they appear to have studiously avoided, in all their writings on the subject, ever giving even a hint as to the probable distance traveled to and fro by the particles constituting a sound-wave, though scores of times recording the actual wave-length in feet and inches! I leave the reader to characterize this kind of "science" as it deserves.

The fact is, physicists have supposed this hypothesis of "wave-length"—so easily deduced from the number of vibrations of a sounding body in a second, by dividing it into the observed velocity of sound—to be a harmless piece of mechanical calculation, which would assist in giving form and definiteness to the wave-theory without endangering its existence or being liable to be turned against it; though even this will soon be seen to be a fatal mistake. So long as "wave-length" alone was involved, the problem seemed amiable and safe. A definite and measurable *amplitude*, however, or even an approximate length of "excursion to and fro" of the wave-particles, in literal *feet* and *inches*, had no such an inoffensive look to these sage investigators! They evidently saw the faint outlines of a *cat* of considerable proportions concealed within this scientific meal-tub of wave-amplitude; and, like the intelligent old rat in the fable, intuitively concluded to keep at a respectful distance, acquiescing in his general opinion that "caution is the parent of safety." They saw, in plain language, if they should allow their "science" to extend far enough to commit the vital act, and thus chain them even to as small an amplitude as the *hundredth part of an inch* for the "excursion to and fro" of the air-particles in a wave-length of "4 feet 4 inches," that it would necessarily and at once involve the same length of "excursion to and fro" of the *iron*-particles in the passage of an *iron* sound-wave of the same length, which would be on its face too preposterous a supposition even for this unspeakably impracticable theory. Hence, the safest way appeared to be to circle all around the meal-tub, but never to directly approach it,—to talk vaguely all around this ugly-looking question of "amplitude" and this so-called "excursion to and fro," and in a non-committal kind of way speak of "wave-particles" as having "comparatively a large amplitude of vibration" and of their swinging "to and fro with the motions of pendulums," and all this; but not to perpetrate the fatal deed of recording the exact or even approximate distance this "excursion to and fro" signifies in any single instance! This was a wise policy in physicists, if even a cowardly one; *but not wise enough*, as the sequel will soon show.

Why have not physicists come out frankly, as candid scientific investigators, and said that "since the only wave-motion we can see and measure has an unvarying proportion of amplitude to wave-length of about 1 to 10, it would seem that sound-waves, if they occur at all, ought to have a similar proportion, or else they are not waves in the proper sense, since they should be essentially identical. And as any appreciable amplitude in iron or other solid body is out of the question, even to the extent of a proportion of 1 to 1,000,000, notwithstanding sound must necessarily travel through it on the same principle as through air, it would seem unavoidable that some other law than wave-motion must be resorted to in accounting for the radiation, propagation, and conduction of sound."

Such a fair and candid statement of the case as this on the part of Professors Helmholtz and Tyndall would have been worthy of the cause of scientific research, and would at once have commanded the respect of the world. Instead of this, however, knowing as they must know that all water-waves necessarily have an amplitude of about one tenth of their wave-length, and knowing at the same time that so-called sound-waves in iron or any other visible substance are destitute of all perceptible amplitude, or any motion whatever to and fro of their particles, yet they go on assuming the wave-theory of sound as established, while flatly telling their readers that sound-waves are "essentially identical" with and "precisely similar" to undulations on the surface of a body of water! Candor compels me to say that this is a fair specimen of that boasted "science" which is to revolutionize the world and overthrow religion!

But we have not yet reached the enormity of this "scientific" idea of "wave-length" in the passage of sound through different substances. The more startling feature of the stupendous fallacy is yet to come.

We have just seen, as quoted from Professor Tyndall, that a tone with 256 vibrations to the second has a wave-length in air of "4 feet 4 inches." But what would be the wave-length of this same pitch of tone passing through a mass of *iron?* Did physicists ever think of this? If they did, they must have done so with their mental eyes shut, and their reasoning faculties half stupefied, or they would have at once realized its ruinous effects upon the wave-theory. Such a tone passing through iron would have a wave-length *seventeen* times as great as in air, or just *seventy-three feet eight inches* from crest to crest! Are such iron-waves reasonable or possible?

The reason for this increased wave-length in iron is plain. Sound passes through *iron* with a velocity *seventeen times greater* than through *air;* and hence the first sound-wave leaving an instrument held against a mass of iron must necessarily travel seventeen times further before the second wave starts than it would have done in air. Hence, sound-waves in iron are necessarily seventeen times as long from crest to crest, or, as these learned physicists prefer it, "from condensation to condensation, or from rarefaction to rarefaction."

I am not guessing at these data when I say that sound passes through *iron* with *seventeen* times greater velocity than through air. Professor Tyndall says:—

"The velocity of sound in water is more than four times its velocity in air. *The velocity of sound in iron is seventeen times its velocity in air.* The velocity of sound along the fiber of pine wood is ten times its velocity in air."—*Lectures on Sound,* p. 47.

But now we reach the culmination of this enormous fallacy. The low E of the double bass has 40 vibrations to the second, which, divided into 1120 feet, the velocity of sound in air, gives its atmospheric wave-length as 28 feet exactly. By holding this instrument against a mass of iron, therefore, and allowing its sound-waves to pass through it, traveling as they necessarily do seventeen times faster than in air, these iron-waves are found to have the prodigious length of *four hundred and seventy-six feet from crest to crest!* Does any man in his senses believe the existence of such *iron-waves* possible, I care not how small the amplitude or so-called "excursion to and fro" of these iron-particles may be? If he does not believe it, then he does not believe in the wave-theory of sound at all;

for this, as every tyro in science knows, is just as true as any other part of the theory.

Thus ends all this courageous talk of Professors Tyndall and Helmholtz about the actual "*wave-length*" of determinate sounds in feet and inches, which looked so harmless on paper, and appeared in the distance to be nothing but *meal;* but which has turned out to be one of the most destructive and prodigious *cats* ever seen in science!

The serious part of the trouble, however, for the wave-theory is still in abeyance. *Amplitude* will not down at the wish or bidding of any physicist. It asserts its claim to recognition and its right to oscillate "to and fro" in every wave, of whatever substance constituted, and refuses to be lugged clandestinely, at the behest of Professors Tyndall, Helmholtz, and Mayer, into incompatible relationship with pretended waves, which are a bald scientific sham. It will not allow its identity to be ignored or obscured. These assumed iron sound-waves, having an indisputable wave-length, according to the current theory of sound, of *four hundred and seventy-six feet,* as every physicist will at once admit, which are "essentially identical" with water-waves and move "exactly in the same way," must necessarily have an amplitude of corresponding proportion to wave-length, the same as in water, if they exist at all; and the iron-particles constituting these enormous billows must therefore make a proportionate "excursion to and fro" as in the case of water-waves of similar length, or they are not "essentially identical" with them, can not be "precisely similar," and do not propagate themselves "exactly in the same way"!

To admit the existence of such iron sound-waves 476 feet long from crest to crest, which are "essentially identical" with water-waves, and then quietly ignore or explicitly deny all practical amplitude, when it is well known that no water-wave can exist at all without a visible and measurable amplitude proportioned to its length as about 1 to 10, would be a quibble and trick unworthy of science, and only supposable in a pettifogging barrister in case of some desperate extremity.

Hence, we reach the logical mechanical conclusion that sound-waves from the low E of the double bass, passing through a mass of iron with a wave-length of 476 feet, must of necessity have an amplitude, making the proportion as 1 to 10, of 47 feet from crest to sinus; or, in other words, the particles of iron constituting the entire mass permeated by the sound must keep up an "excursion to and fro" a distance of 47 feet, making 40 of these complete oscillations every second!

If there was anything strained, exaggerated, or unfair, about this argument, or the slightest misrepresentation of the teaching of physicists, or misstatement as to the laws and principles of science involved, it would certainly be a great relief to Professors Tyndall and Helmholtz in this terrible ordeal of their favorite theory. But even this poor consolation is denied them. They are compelled to stand awe-struck and speechless in the presence of these prodigious sonorous billows permeating a mass of iron *four hundred and seventy-six feet long* "from condensation to condensation," and *forty-seven feet high* from the top of the compressed ridge to the bottom of the rarefied furrow, with all the iron-particles composing the mass rushing "to and fro with the motions of pendulums"! To deny the existence of such iron-waves, at least 476 feet long, is to deny the truth of the wave-theory altogether, either as relates to air or any other substance; while to deny this proportion of amplitude or "width of swing" of 47 feet in billows having such an admitted wave-length is for Professors Tyndall and Helmholtz to repudiate their own language, and proclaim to the world that there is no sort of resemblance between water-waves and so-called sound-waves, instead of them being "essentially identical," "precisely similar," and traveling "exactly in the same way."

The question of questions on this subject, then, is, will these eminent authorities, in view of such overwhelming facts, abandon the wave-theory of sound as a practical and self-evident absurdity, and accept in its place the beautiful and every way consistent hypothesis of substantial corpuscular emissions? We shall see.

But we are not yet done with this question of amplitude. No physicist, after his attention is called to the question, will pretend to doubt the correctness of the calculation here made as to such sound-waves in iron having an actual wave-length of 476 feet from "condensation to condensation," or from "crest to crest," if the phrase suits better; that is, if the mass of iron is large enough. Either Professor Tyndall or Helmholtz would admit at once, if asked by any one, that, according to the principles of the wave-theory, the sound of the low E of the double bass would have the wave-length in iron just as given in my calculation. But while admitting this, what would they or could they say about *amplitude?* They would unquestionably be obliged to admit some amplitude, or evidently they would not be waves at all, since manifestly a water-wave without amplitude would be without crest or furrow, and hence a nonentity.

Professor Tyndall could not get away from his own words, already quoted, even if he wished to, that "*during the passage of the wave every particle concerned in its transmission makes only a small excursion to and fro,*" and that "the *length* of this *excursion* is called the *amplitude of the vibration.*"—*Lectures on Sound*, p. 44.

We must constantly bear in mind that there can be but one mode of sonorous propagation through any substance, according to the wave-theory, and that is *wave-motion*,—that while waves on the surface of a body consist of crests and furrows, waves in the interior of a mass, whether it be *air*, *iron*, or any other substance, have been defined over and over again by these writers as consisting of "condensations and rarefactions" of the materials constituting the waves, while these again have been as clearly described as the alternate *squeezing of the particles more closely together and separating of them more widely apart*, thus causing this "small excursion to and fro" which constitutes the "amplitude of the vibration," making it the same practically, so far as motion and amplitude are concerned, as if the waves were produced on the surface of the body, and took the ordinary form of crests and troughs. Hence, an iron sound-wave, whether on the surface of the mass as a "crest and sinus," or formed as a "condensation and rarefaction" in its interior, must possess the same "amplitude of vibration," "width of swing," or "excursion to and fro" of the iron wave-particles as a similar wave would have in air, or there is no consistency nor congruity in the theory, and all this talk about "condensation," "rarefaction," "excursion to and fro," "width of swing," "amplitude," even "wave-motion," is an imposition upc the scientific public.

I now ask Professors Tyndall and Helr holtz,—and hereby send my inquiry acrc the Atlantic Ocean,—if the wave-theo be true, and if there be such a thing sible as a *sound-wave* in any substance, *what is the amount of this "amplitude of the vibration," or the length of this "excursion to and fro," or "width of swing" of the particles constituting a sound-wave in iron?* Answer something, if it is but the millionth of an inch! Don't, for the sake of science, be non-committal any longer! Silence and candor are wholly incompatible on such a vital question as this. If the iron-particles move at all, or make the least possible "excursion to and fro," as so distinctly taught by the current theory of sound, say so; and if they do not, say so; and then abandon the wave-theory! I pause for a reply.

But the reader, I imagine, will not pause or be satisfied to wait to hear from the other side of the ocean. He wants the matter to be settled at once. Hence, I must answer for Professors Tyndall and Helmholtz till they shall have time to speak for themselves. My answer is as follows: *This assumed amplitude in iron sound-waves, or this so-called "excursion to and fro" of the particles of iron constituting these billows, is practically nothing, and they know it!* That is, to use their own language when closely pressed, it is "infinitesimal," if it is anything at all, since the most powerful microscope ever constructed fails to reveal the slightest molecular movement in a mass of iron, or any other solid or liquid substance, permeated by the intensest sounds. Hence, it is within the truth to say that these supposititious sound-waves are absolutely devoid of amplitude, and therefore are not *waves* at all!

Here then, reader, according to this theory, we have the grand scientific (!) spectacle of iron *billows* with an actual and admitted "wave-length" of *four hundred and seventy-six feet, and no amplitude!* Yet these physicists call them "*waves*" with a license unparalleled for its absurdity!

To realize the enormous character of the fallacy here being exposed, the reader has only to imagine, if he possibly can, *ocean billows* (which are always referred to by writers on this subject as appropriate illustrations of sound-waves) having the prodigious wave-length of *four hundred and seventy-six feet* from crest to crest, and a depth of furrow—well, say, of *one inch!* Though this would be a ridiculous caricature on wave-motion, yet such furrows would be a million times deeper than the furrows of these boasted sound-waves in iron, if they possess any amplitude at all, notwithstanding their acknowledged wave-length of nearly a tenth of a mile! And knowing all this to be true, as we must assume to have been the case with these representative scientists of the age, how can we account for the reiterated language already quoted in comparing sound-waves and water-waves—"*essentially identical,*" "*precisely similar,*" moving "*exactly in the same way,*" while one lacks *amplitude*, the only thing, in fact, which constitutes a wave in any substance?

But if such a pitch of sound as I have assumed passes through iron in this way, having an actual wave-length of 476 feet and a depth of "amplitude" so "*infinitesimal*" that the most powerful magnifying glass fails to reveal it, then how much, I ask, does it lack of a straight course? If a line were drawn 476 feet so nearly straight that a powerful microscope could not reveal the least deflection, is there a mathematician on earth who would not, without a moment's hesitation, pronounce that a *right line?* Am I not justified, therefore, when I assert that so far from sound passing through rock, iron, water, wood, or even air, by wave-motion (which has no existence at all without *amplitude*), its route can only be a direct line?

And if it is practically and mathematically a *straight line*, it is exactly what the corpuscular hypothesis requires and teaches, namely, that sound passes through all bodies in the form of sonorous pulses radiated from the sounding instrument in *straight lines*, and that these primary systems of corpuscles radiate secondary systems also in *straight lines*, these others, and so on, permeating all parts of the conducting medium, whether that be air, water, wood, or iron. Which, I now appeal to the intelligence of the reader, is the more consistent and rational system? That which encounters no contradiction and no absurdity, or that which is only contradiction and absurdity from beginning to end?—which admits sound-waves in iron to be 476 feet long, telling us at the same time that "sound-waves" move "*exactly in the same way*" as water-waves, are "*essentially identical*" and "*precisely similar*," but which turn out, on examination, to have no *amplitude* (the only thing that really constitutes a wave), not even amounting to the millionth of an inch! I might well stop here, and risk the result of this investigation without submitting another point, letting the fate of the wave-theory hinge upon this single argument. But I have an abundance of other considerations equally pertinent and unanswerable, some of which will be even more surprising to the unscientific reader.

One would think that a competent scientific investigator ought to see at a glance that the physical motion of a gross body, like iron, if *too small to be observed* when the eye is aided by the microscope, must be *too small to sensibly affect any other sense-nerve*. Surely the eye is the most sensitively acute of all the senses in perceiving that which comes within its proper scope, such as the *motions* of a physical visible body. It is a fact undeniable that a movement a thousand times smaller than could be possibly recognized by touch in the most sensitive portion of the human organism, could be readily seen under a powerful magnifying glass. Is it reasonable, then, that the *motion* of a visible body (for it can be only *motion* according to the wave-theory) which eludes the recognition of this most searching sense, thus aided, should address and impress another sense entirely unaided, which is surely not so acutely adapted to the phenomena of motion in physical bodies as either sight or touch? It must seem, therefore, viewed from every possible standpoint, unphilosophical and in violation of all true science to designate as *wave-motion* a supposed movement in the particles of a gross physical body, which has never been observed under the strongest magnifying power, particularly when such hypothetic movement is unnecessary for the solution of any problem in science, and especially in view of the probable truth, not to say beautiful consistency, of the corpuscular hypothesis, which necessarily involves the propagation of sound in *straight lines* through all bodies, and which the wave-theory is at last compelled to admit.

I now propose, in concluding this phase of the argument, to show that physicists, in thus referring to *water-waves* as illustrative of *sound-waves* in air, have necessarily and unmistakably abandoned sound-waves altogether, either in air or in any other conducting medium! This surely will be more than these astute writers on science contracted for in their careful analysis of water-waves, and their studied efforts to show how the superposition of tiny wavelets, traversing the surface of large rollers, corresponds to the superposition of air-waves, constituting sound and making up the "algebraical sum" of their different systems of wave-motion.

The truth is, these writers, in their enthusiasm on the subject of air-waves as the cause of sound-propagation, and in their usual habit of jumping at conclusions, appear to have rushed headlong, so to speak, not stopping to think where their argument would lead them, or what would be the consequence when their reasoning should force them up against a mass of rock or iron, or into a body of water, which admits of palpable and visible investigation.

A more reckless and short-sighted course of argumentation perhaps was never adopted or recorded even in the crudest scientific speculations of any half-civilized philosopher of ancient or modern times. Air being wholly invisible and almost intangible, these eminent investigators have felt safe in bravely assuming its particles as oscillating "to and fro with the motions of pendulums," and as having "comparatively a *large amplitude* of vibration," and all this, because no one could see to the contrary, and therefore they seemed intuitively to think that no one could contradict them! But this superficiality, like that of the Ptolemaic philosophers, has at last to meet its fate, since this reasoning explodes itself, as we have seen, the moment the "*large amplitude of vibration*" and "excursion to and fro" are carried into a mass of visible iron, having sound-waves just *seventeen times longer* than in air, and consequently which should have seventeen times this "large amplitude of vibration," according to all laws of symmetrical proportion governing water-waves, which are so repeatedly claimed to be "essentially identical" and to move "exactly in the same way"!

But here comes, as just intimated, what I consider the utter abandonment of the idea of sound-waves, either in air or in any other substance. When Professors Tyndall and Helmholtz were so confidently illustrating sound-waves in *air* by the action of "water-waves" which were "essentially identical," they appeared absolutely to forget, for the time being, that *sound traveled through water at all!* This unfortunate slip of memory now proves ruinous to their theory, *since a sound-wave in air being of course* and admittedly nothing more nor less than an *air-wave*, it follows therefore that a sound-wave in water must necessarily be nothing more nor less than a *water-wave!* There is no escape from this. If a sound-wave in water does not constitute a *water-wave*, in the true and literal sense, then it becomes demonstrative proof that a sound-wave in air does not constitute an *air-wave* at all, and consequently the bottom falls out of the wave-theory. But as universal observation assures us that a sound, however intense, passing through water does not produce the slightest undulatory effect, or stir the particles of water through which it passes, it follows that *wave-motion* in both *air* and *water* has broken down!

Every one knows what a "water-wave" is, and that it has no double or doubtful meaning. Fortunately, in *water* we do not need these mysterious and almost meaningless "condensations and rarefactions" so essential to the wave-theory in fabricating hypothetic *air-waves* in the midst of the "aerial ocean," which seems to grow out of the fact that *we can not get at the surface of the atmosphere.* In water we have an actual, tangible, ponderable liquid, with a visible surface on which "water-waves" are easily produced and visibly observed. And hence, if Professors Tyndall and Helmholtz speak of a "water-wave," we know exactly what they mean, namely, an undulation on the surface having a visible crest and sinus, with an actual amplitude, which oscillation to and fro has invariably a proportion of about 1 to 10 of wave-length. Hence, when they assure us, as they so often have done, that a sound-wave in air is "essentially identical" with a "water-wave," we have to understand, as a matter of course, that a sound-wave in *water* is also "essentially identical" with a "*water-wave*"! This must be so, or there is no meaning in the scientific teaching of these physicists. But as no "water-wave" is produced by sound passing through it, even under microscopic observation, it shatters the whole wave-theory, and proves that *air-waves*, as the result of sound, are just as fallacious as "*water-waves.*" Can anything be more conclusive than this?

Physicists will hardly venture to resort to the disingenuous quibble that there are two distinct kinds of water-waves,—one kind *visible* and the other *invisible*,—one kind with *crests, furrows, wave-lengths*, and *amplitude*, the other kind with *wave-lengths* but with neither *crests, furrows*, nor *amplitude*; and that these *invisible, inscrutable*, and *crestless* water-waves are the ones produced by sound, while the *visible* and *measurable* waves are the kind produced by throwing a stone upon the surface of a piece of calm water! If they really should venture to assume any other class of water-waves than *visible* ones, such as everybody understands by the term "water-wave," it would have been a good thing in their repeated use of the term in their works on sound to prefix some sort of qualifying word when speaking of "water-waves," that their readers might not be at a loss to know which class of waves they referred to! For example, when speaking of a sound-wave in air being "essentially identical" with a "water-wave," and traveling "exactly in the same way," the reader is obliged to ask, "Which class of 'water-*waves*'?—those with *crests* and *troughs*, or those without?" By having neglected this precaution they naturally leave us to infer that there is but one class of "water-waves," as every one understands, and as they themselves know! In fact, it is little less than inexcusable negligence, if these physicists ever intended to teach more than one kind of "water-waves," that they should have studiously kept it to themselves, and never once given an intimation of such *crestless* and *invisible* billows in water, with wave-lengths from 10 to 100 feet!

Seriously, this convenient *invisible* dodge can be played in *air* to almost any extent, since the motion of its particles is not observable; but it will turn out about as much of a scientific failure when attempted in *water* as it has done in *iron*, with billows having a wave-length of 476 feet but with an amplitude so small that the most powerful microscope fails to reveal a trace of it! Such invisible shifts will prove also too shallow in water. It is a well-known fact that sound travels through water with over four times the velocity as through air, and hence with over four times the wave-length from crest to crest. Yet not a semblance of wave-motion or any other motion can be detected in water from the action of any sound passing through it, even with the aid of the microscope, notwithstanding a *sound-wave* is "essentially identical" with a *water-wave*, which always has an amplitude or a "to and fro" motion of its particles an actual distance equaling one tenth of the wave-length.

But even supposing there was another class of "water-waves" possible as the product of sound, what difference could it make with my argument? None at all, since such sound-waves in water would still be "essentially identical" with the visible waves caused by throwing a stone upon its surface, and would move "exactly in the same way"! It surely would do the wave-theory no good, therefore, to resort to such hypothetic "water-waves" as being produced by sound, after admitting that they are "precisely similar" to "water-waves" produced by a stone, and that they are propagated "exactly in the same way." That "the way of the transgressor is hard" is no less a truism in science than in religion!

To show that sound in passing through water does not produce the slightest wave-motion in the interior of its mass, we have only to take a glass jar of water charged with some kind of coloring matter which will float through it in granules, and then examine it with a microscope under a strong light, while holding the stem of a tuning-fork in the water. That the sound of the vibrating fork permeates the water and passes through it in all directions is evident, since it is conducted to the table on which the jar sits, and is caused to ring out by its resonance with augmented volume. Yet the particles of coloring matter suspended in the water do not stir nor go through the least perceptible oscillation.

We see none of Professor Mayer's swinging "to and fro with the motions of pendulums," nor of Professor Helmholtz's "comparatively large amplitude of vibration," nor of Professor Tyndall's "small excursion to and fro"! Yet the sound-waves produced by this tuning-fork in water are more than four times as long as the waves in air would be from the same fork, according to the wave-theory, and hence the "excursion to and fro" in water, if there is any such excursion, should be over four times as large as in air! If there is any truth in the wave-theory, and if sound travels through water by means of wave-motion, why do not the floating particles in the water permeated by sound show some sign of oscillation?

It is true a visible circle of delicate waves may be seen on the surface of the water of the jar directly around the fork; but, as I have repeatedly explained in the preceding chapter, this is purely incidental, as the effect of the tremulous movement of the tuning-fork's stem, and not as the result of the action of sound at all. But since these learned physicists are just about superficial enough, as proved by their general investigations on this subject, to make a point of this diminutive wave-motion produced by the stem of the fork, I had better meet it in advance, and once for all, in a single brief paragraph, as follows:—

As a proof that these tiny wavelets are not "sound-waves" at all, let us suppose the fork to have one hundred vibrations in a second. By actual observation the wavelets sent off from its stem over the surface of the water are found to have a wave-length of *not over an eighth of an inch from crest to crest;* whereas, if they were really sound-waves, or even "essentially identical" with them, they would necessarily have a wave-length between 40 and 50 feet from crest to crest in water, or 11 feet 4 inches in air, as every physicist at all conversant with the current theory well knows! Thus, the only plausible argument or appearance of one in favor of actual sound-waves in water (for which the theory is indebted to my own experiment) has been ingloriously exploded in advance!

But the final and overwhelming evidence that "water-waves" can not, by any possibility, constitute sound-waves, or be the means of sonorous propagation in water, is drawn from the fact that if we throw a stone, weighing a pound, for example, into a piece of calm water, its waves will only travel at a velocity of *three feet a second,* as ascertained by careful observation and measurement; while sound, as recently quoted from Professor Tyndall and as all authorities agree, travels in water with a velocity of fully 4,500 feet a second, or *fifteen hundred times faster than visible water-waves!*

Is it reasonable or conceivable that one system of "water-waves," caused by a *stone*, should be "essentially identical" with another system of "water-waves" caused by a *sound*, and that both systems should be propagated "exactly in the same way," while one system travels *three feet in a second*, and the other system *four thousand five hundred feet in the same time?*— one system having always an amplitude of about *one tenth* of its wave-length, while the other system, though it may have the same definite wave-length in feet and inches, yet has no amplitude at all?—one system of waves being visible to the naked eye, even if its wave-length be only the quarter of an inch from crest to crest, while the other system, even with a wave-length of over a *hundred feet* can not be seen at all under the magnifying power of the microscope? The absurdity of the idea glares contemptuously into the faces of modern physicists.

Hence, we reach the most demonstrative proof that sound does not and can not travel in water by wave-motion at all, since these measurable waves—the only class of water-waves ever observed—have but the *one fifteen hundredth* the velocity of sound!

If these candid investigators of physical science should claim, as just discussed, some other kind of water-waves not visible to the naked eye, or even by the aid of the microscope, which might possibly have a greater velocity than the above, or travel more than *three feet in a second*, such waves, as already shown, would evidently do their theory no good, *since they would not be sound-waves at all*, according to their own repeated statements, *unless they were "essentially identical" with visible "water-waves," and traveled "exactly in the same way"!* Thus, the closer we follow up this question, and the more rigidly we pin down these learned authorities to their own voluntary admissions, the more hopelessly demoralized the wave-theory becomes.

The conclusion is thus unavoidable that sound produces no wave-motion whatever, either in air, water, iron, or any other conducting medium, whether it be solid, liquid, or gaseous; but must travel through whatever medium conducts it in straight lines, according to the beautiful and consistent laws and principles unfolded and enunciated by the corpuscular hypothesis.

I could extend this argument, based on the analogy drawn from water-waves,—the only basis for any correct scientific knowledge of wave-motion,—but I have concluded to reserve the most crushing of all the arguments against the current theory of sound, based on such analogy, as a suitable and demonstrative culmination of this monograph.

In view of facts thus hastily passed in review, and especially in view of sound-waves in iron 476 feet long from "condensation to condensation," yet without amplitude, according to the teaching of physicists and as an unavoidable concomitant of the wave-theory, it becomes impossible to even attempt a rational explanation of the marvelous want of perspicacity in scientific investigators which has not permitted one of all the thousands who have studied the phenomena of sound to even suspect the manifest fallacy of a theory so fraught with impossibilities and absurdities. It wholly surpasses comprehension that among the greatest analytical thinkers the world has ever contained,—those particularly accustomed their lives long to searching and critical investigations,—not one has been found to expose the laughable weakness and pitiable puerilities of this hypothesis, with so many self-evident impracticabilities confronting it, which, on their bare mention, demonstrate it to be one of the most enormous scientific errors of this or any other age.

In presenting these sonorous difficulties to a scientific friend—by the way, a firm disciple of Professors Tyndall and Helmholtz—he promptly confessed the absurdity of actual iron-waves, with "condensations and rarefactions" and a "small excursion to and fro" of the real particles of iron throughout the "amplitude" of the wave-motion, and suggested, as a probable and reasonable way to escape the difficulty and still believe in the wave-theory, the supposition that it might be the *air* in the iron which served as the undulatory medium for sound-propagation, since all bodies are porous, and contain more or less air. But this was instantly shown to be untenable by referring to Professor Tyndall's *Lectures on Sound*, where he gives tables showing the velocity of sound in all kinds of metal, wood, liquid, and gas, according to their density and elasticity, in *contradistinction to its velocity in air*, showing that sound-waves are thus admitted to be composed of iron, rock, wood, water, and gas, when passing through them, just as they are composed of air-particles when passing through air!

Besides, if it was *air* in the iron instead of the iron-particles themselves which constituted the sound-waves, how does it happen that sound travels *seventeen times faster* in iron than in air, as calculated by such scientists as Newton, Laplace, Chladni, Savart, Despretz, Helmholtz, and Tyndall? (See Tyndall's *Lectures on Sound*, p. 39.) As all these substances just named are placed in contrast with air, each transmitting sound-waves with a different velocity, it is no more logical or reasonable to claim that it is the air in iron which furnishes the undulatory motion for sound than to suppose it to be the air in *hydrogen gas* which meets the same necessity, since sound passes nearly four times faster through such gas than through air!

But this attempted evasion is utterly overthrown by the fact that sound passes through water from which all air has been extracted by heat with *four times the velocity of its propagation in the atmosphere*, proving that sound-waves in any solid or liquid body, if they occur at all, must be constituted of the absolute particles of such conducting medium.

Thus the question of sound-propagation was left with my friend in a state of hopeless demoralization, because it was impossible, as he thought, for Tyndall and Helmholtz to be wrong, and it was equally impossible for sound to go through solid iron in waves, with "condensations and rarefactions" and a "small excursion to and fro" of all the iron-particles composing such waves, especially such inconceivable waves as those required by the theory —*four hundred and seventy-six feet long* from "condensation to condensation"! I left him, therefore, with the incubus of an iron billow the tenth of a mile long, having a crest or "condensation" forty-seven feet high, pressing on his mental vision, but with a promise to candidly investigate the subject and report at our next meeting.

To my surprise, I found him at the next interview cheerful and light-hearted, having evidently shaken himself free from the fearful load left on his mind a few nights previously. He now was able, he declared, to solve the problem of sound passing through iron in waves of any required size and dimension without the aid of air, and without the fatal and pulverizing necessity of the "small excursion to and fro" of the iron-particles constituting the wave. He also had discovered, he asserted, an important solution of the problem of sound passing through air in *real waves*, which would obviate the enormous absurdity of a locust compressing four cubic miles of atmosphere sufficiently to add one sixth to the velocity of sound, thus exerting the energy of more than fifty million horses! With astonishment I awaited the unfolding of the new hypothesis, which was to save the wave-theory from hopeless disaster and give a new lease of life to a philosophical doctrine which I had, as I conceived, utterly demolished.

My friend then proceeded to divulge the important secret of his discovery, namely, that sound passes through all substances, even through air, by means of *ethereal* undulations,—that it is not the *air*, nor the *iron*, nor the *water*, nor the *gas*, which is thrown into waves by the action of sound, but the *ether* which permeates all bodies, and which constitutes the undulatory motions which we term *light* and *heat*. Hence, he contended earnestly and enthusiastically that there was not the least difficulty in a locust filling four square miles with undulations of this substance, which was probably a thousand million times less dense than the most attenuated gas, while not the least absurdity would be met with in sound passing through iron, with waves a quarter of a mile long, having an amplitude of a hundred feet if necessary, since such undulations, instead of disturbing the texture of the iron in the slightest degree, were only the molecular movements of that *ether* which circulates freely through the substance of a diamond, and without which light could not exist!

The reader may guess the Doctor's consternation when this marvelous scientific palace of Aladdin was caused to fall into shapeless rubbish at his feet by touching *it* with the wand of a single fact which the whole scientific world admits, namely, that *sound will not pass through a vacuum at all, while a vacuum is just as certainly filled with this hypothetic ether, since light passes as freely through a vacuum as through air!* Thus, by a single touch this beautiful *ethereal* castle in the air fell to the ground.

Besides this annihilating fact, I referred him to the conclusive argument just employed with reference to air in iron as the means for producing sound-waves. If *ether* pervades all bodies, and if sound-waves are only *ethereal* undulations, why should sound travel seventeen times faster in iron than in air? It is evident that there is more room for *ether* in air than in a dense body like iron. It therefore turns out, according to this brilliant discovery, that the less the quantity of ether the greater the velocity of sound,—which, carried far enough, would prove that if there were no *ether* at all the velocity of sound would be still greater! Thus, it turned out that this important discovery of my friend had just about as much weight as the substance on which it was based.

To satisfy the Doctor as to this terrible demolition of his grand creation, I then turned to Professor Tyndall's work on "Sound," and read numerous passages in which he distinctly and unequivocally teaches that it is the "*air-particles*" themselves which are "moulded" into "waves," with "condensations and rarefactions," and which actually make the "small excursion to and fro," and that it is the physical atmosphere which is thus *heated* by the passage of these sound-waves, and its "temperature" so raised as to actually increase its "elasticity" "one sixth," by which "one sixth" is added to the velocity of sound. I also showed by these quotations that Professor Tyndall (my friend's great mentor) never dreamed of *ether* in the air being the medium of sound-waves, and hence that *ether* can not so act in iron, because he particularly shows on page 7 of his treatise on "Sound" that although a vacuum is full of *ether* yet sound can not travel in it. Among these quotations overthrowing this *ethereal* palace of my friend were the following, some of them already quoted on pages 78 and 79:—

"Figure clearly to your minds a harp-string vibrating to and fro; it advances, and causes the *particles of air* [not particles of *ether* or some other element existing in the air] in front of it to *crowd together*, thus producing a *condensation of the air*. It retreats, and the *air-particles* behind it *separate more widely*, thus producing a *rarefaction of the air*. . . . In this way the *air through which the sound of the string is propagated is moulded into a regular sequence of condensations and rarefactions which travel with a velocity of about* 1100 *feet a second*." —"The pitch of a note depends solely on the number of *aerial waves* which *strike the ear* in a second. [Showing that these "*aerial waves*," which are "moulded" by the string, actually travel the whole distance within which the sound is heard, if a dozen miles, since such *waves* "strike the ear."] The loudness or intensity of the note depends on the *distance* within which the *separate atoms of air vibrate*. This *distance* [Mark it, a real "*distance*," increasing according to loudness or intensity,] is called the *amplitude of vibration*."—"We have already learned that what is *loudness* in our sensations, is, outside of us, nothing more than *width of swing*, or amplitude of the *vibrating air-particles*." —"Imagine one of the prongs of the vibrating fork swiftly advancing; it compresses the *air* [not the *ether*] immediately in front of it, and when it retreats it leaves a partial vacuum behind. . . . The whole function of the tuning-fork is to *carve the air* [not carve the *ether* or some other substance] into these *condensations and rarefactions*."—TYNDALL, *Lectures on Sound*, pp. 48, 62; *Heat as a Mode of Motion*, pp. 225, 372.

I then proved to the Doctor that his favorite physicist, Professor Tyndall, was not alone or peculiar in thus teaching that sound-waves were constituted of the real particles of the substance through which they pass, by taking down from his own magnificent library numerous authors who teach exactly the same thing. In the article on "Sound," for example, in Appleton's *American Encyclopedia*, Professor Mayer, a high authority, distinctly teaches that it is the air-particles themselves which, in a sound-wave, have a regular isochronal movement, and "*swing to and fro with the motions of pendulums*" as the sound travels, keeping up the same oscillations "to a distance." Professor Mayer remarks:—

"It is evident that the ultimate effect of the passage of *sonorous waves through the atmosphere* will be to cause the *molecules of the air* [not the molecules of *ether*] *to swing to and fro with the motions of pendulums*. It is also apparent that all the characteristics of the periodic motion at the source of the sound *will be impressed on the surrounding air, and transmitted through it to a distance*."

I also referred him to Professor Helmholtz, where he distinctly teaches that in the passage of a sound-wave through the air the particles of the atmosphere—not of the *ether*—take on "comparatively a *large amplitude of vibration*," as recently quoted.

In addition to these, and numberless passages which might be quoted from high authorities on the subject, I pointed out to my friend the fact that in Professor Tyndall's *Lectures on Sound* he devotes several pages (26 to 37 inclusive) to an elaborate calculation, condensed from Laplace, the great astronomer and mathematician, to show why sound travels through air at the freezing temperature 1090 feet a second, notwithstanding Newton's basis of sound-velocity, deduced from the density and elasticity of the air, proves that it can not exceed 916 feet a second. Professor Tyndall accounts for this difference of 174 feet a second (about one sixth) between Newton's law and the observed velocity, by the hypothesis so often quoted, that all sounds in passing through the atmosphere produce waves which cause "condensations" of the air, and thus generate *heat* throughout the entire distance the sound travels, and that this augmentation of the air's temperature increases its "*elasticity*," which makes up the discrepancy in Newton's calculation by adding *one sixth* to the velocity of sound. In all this elaborate calculation by Professor Tyndall, too long to quote, the operation is shown by an engraving to be performed by the actual *air-particles* first pressing forward into one portion of a wave where they become heated by pressure, and then oscillating backward into another portion where they become cooled off.

From all this, I showed him that it was simple folly to try to evade the fatal consequences of wave-motion, which explicitly inculcates that the actual particles of the substance through which sound passes—whether it be air or iron, wood or water, —*constitute the undulations*, and literally make up the "small excursion to and fro" as each sound-wave passes; and that any serious effort by a physicist to evade this consequence would be to abandon the whole wave-theory.

I was thus exorbitantly particular on this point of the *wave-particles* themselves actually making the "excursion to and fro," and in showing that I did not misconceive nor misrepresent the wave-theory, that by no possible contingency should the appearance of a quibble or evasion intervene to save the scientific monstrosity from destruction. At the close of this second interview I had the satisfaction, if not of fully converting my friend to the new hypothesis of substantial sonorous pulses, at least of obtaining from him the voluntary admission that such a thing as literal undulations in iron by the passage of sound, causing its particles to oscillate "to and fro with the motions of pendulums," to say nothing of iron billows with a wave-length of *four hundred and seventy-six feet* from "condensation to condensation," which the theory necessarily requires, was too infinitely preposterous a supposition for any scientific mind to entertain for a single moment.

I now assert that it is safe to predict that the elaborate argument and calculation just referred to, in which Professor Tyndall unwittingly proves by careful figures and illustrations that the stridulation of a locust raises the temperature of the condensed half of four square miles of atmosphere, and thus increases its elasticity and adds one sixth to the velocity of sound, will be regarded by future generations as one of the most laughable philosophical curiosities ever placed on record by a sane mind, and by the side of which the Ptolemaic absurdities (of making the earth the center of the universe, with the sun, moon, and stars revolving around it every twenty-four hours) sink into insignificance. While the amused reader, hundreds of years hence, will find no difficulty in framing ample excuse for the Ptolemaic school of philosophers on account of the manifest physical appearances of the heavens, he will be able to find nothing in the scientific literature or the advanced state of mental cultivation of this age of steam presses and lightning telegraphs on which to base the least foundation for an excuse palliating so stupid a theory as this of which Professors Tyndall, Helmholtz, and Mayer are the popular and acknowledged champions,—compared to which the silliest scientific hypothesis of Aristotle becomes sound philosophy.

Take the following as one of the many inevitable results of the atmospheric wave-theory of sound. The hypothesis that each particular tone consists of a regular sequence of air-waves, with condensations and rarefactions which travel in symmetrical succession throughout the distance the sound is heard, sometimes for many miles, without the tone being marred or distorted in the least degree, as distinctly taught by all writers on the subject, is met by the following insurmountable difficulty in the very operation itself,—a difficulty which, when properly weighed, must break down the hypothesis without the aid of another argument.

Waves of water, to which sound-waves are always compared, meeting each other from three or four different directions, will clash together and become broken up, disappearing in an indistinguishable mass of irregular hillocks, without the possibility of an approach toward reconstruction after collision. This is a fact well known to any one who has ever taken the trouble to observe the action of ripples meeting on the surface of a pond from the effect of three or four stones dropped into the water a few yards apart. No possible continuity of symmetrical waves can be traced after such collision and commingling, since a system of waves from one direction could move no farther in regular form and order after meeting a system of equal amplitude from another direction. Much less could twenty such systems of undulations, coming from twenty different directions, meet, clash, and intermingle indiscriminately, and then each series move on as *waves*, undisturbed or undistorted, which is absolutely the case with atmospheric sound-waves according to the current theory, since twenty musical instruments may be playing at the same time in different directions around you, with their sonorous waves necessarily crashing through each other and breaking up like water-waves into manifold and irregular hillocks, yet by an effort of attention the notes of each instrument can be distinctly recognized as pure and unbroken as if nineteen other systems of sound-waves were not dashing through them in different directions!

Need we ask a clearer demonstration that the tones of these various instruments do not consist of air-waves which Professor Helmholtz assures us, as already quoted, move "exactly in the same way" as water-waves, are "essentially identical," and "of a precisely similar nature"? If these sounds were really constituted, each of a "regular sequence" of atmospheric undulations moulded and sent off by its respective instrument, as Professors Tyndall and Helmholtz teach all through their books, it would inevitably follow that not a single tone could reach the ear undistorted, or in its proper vibrational form, if at all, as the waves would surely clash and be broken into a confused mass; for, let it be distinctly remembered that if sound is constituted of *waves*, then, whenever the waves are ruptured or disintegrated, as they would be if a number of systems clashed together, the sound would be changed from musical tones to mere *noise*, if not destroyed altogether! Is not this self-evident to every mind competent to investigate scientific matters, especially in view of the fact that air-waves are "essentially identical" with water-waves?

When on another phase of the sound-theory and when trying to illustrate the operation of his "condensations" and "rarefactions" in creating a "phase of opposition" and producing "interference," Professor Tyndall distinctly teaches that if *only two* equal systems of waves, whether of sound or water, should happen to "interfere" by the *crests* of one system falling into the *furrows* of the other system, they would *mutually destroy each other*. I will quote his words:—

"In the case of *water*, when the *crests* of *one system of waves coincide* with the *crests* of *another system*, higher waves will be the result of the coalescence of the two systems. But when the *crests* of one system coincide with the *sinuses* or *furrows* of the other system, the two systems in whole or in part *destroy each other*. This *mutual destruction* of two systems of waves is called *interference*. The same remarks apply to *sonorous waves*. If in *two systems of sonorous waves condensation coincides with condensation* and *rarefaction with rarefaction*, the sound produced by such *coincidence* is *louder* than that produced by either system taken singly. But if the *condensations* of the one system *coincide* with the *rarefactions* of the other, a *destruction* total or partial of *both systems is the consequence*. . . . If the two sounds be of the same intensity their *coincidence* produces a sound of four times the intensity of either; *while their interference produces absolute silence*."—*Lectures on Sound*, pp. 284, 285.

There is no misunderstanding this citation; for if two systems of equal waves from two unison forks, for example, "*interfere*," by the forks being placed half a wave-length apart, so that the "condensations" from one fork "coincide" with the "rarefactions" from the other, "their *interference produces absolute silence*." Yet, as we see, twenty different sounds, with their twenty different systems of air-waves, will infallibly reach the ear from as many different directions, while each individual sound will be as distinctly heard by special attention and as perfectly unbroken as if no other sounds crossed its path. Is it possible to suppose that twenty different systems of actual, corporeal *air-waves*, from as many points of the compass, can thus crash through each other, but invariably, without a single exception, while being fretted and broken into inexplicable tumuli, as they must be if actual waves, each proceeds separately on its journey, and undistorted enters the ear with its "condensations" and "rarefactions" unmarred, —as must be the case to represent the appropriate tone? Yet *two systems* of sound-waves are just as liable to *interfere* and cause "absolute silence" as to *coincide* and be *heard!*

Nothing, it would seem, but desperation in support of a theory could prevent a mind competent to reason on a scientific subject from seeing the contradiction and practical fallacy of the wave-theory, from this consideration alone. Yet so far from throwing a ray of suggestive light on the mind of Professor Tyndall, so absolutely wedded seem all his intellectual powers to the manifest folly of air-waves, that he not only is willing to accept the stupendous impossibility of twenty such systems of atmospheric undulations breaking through each other and yet continuing undistorted, without the shadow of "interference," but he raises the number to a "thousand" systems of such waves passing through "the same air" "at the same time," and each tone addressing the tympanic membrane, if listened to by the proper act of attention. As there is no possible way of knowing that "the same air" can accommodate a "thousand" tones from a "thousand instruments" at "the same time" only by hearing them, it utterly explodes this idea of the "interference" of air-waves, and with it the existence of such waves as the means of sound-propagation. For, if sonorous air-waves really exist, and if two systems stand an equal chance of destroying each other by interference, what would become of a "thousand" systems from a "thousand instruments" passing through the same air at the same time? Professor Tyndall remarks:—

"The same *air* is competent to *accept* and *transmit* the *vibrations of a thousand instruments at the same time*. When we try to visualize the *motions of that air*—to present to the eye of the mind the *battling of the pulses direct and reverberated*—the imagination retires baffled at the attempt."—*Lectures on Sound*, p. 257.

No wonder "the imagination retires baffled" at the legitimate consequences of a theory so practically impossible and absurd, in the very nature of things! We have only to reflect that the cylinder of air entering the ear is no larger than a straw, and that this small body of air has to receive the *waves* from "a thousand instruments at the same time," and that these are actual, physical air-waves, with "condensations and rarefactions," some of them measuring *five*, *ten*, and *twenty* feet from crest to crest and of proportionate amplitude, each instrument sending into this small cavity from *forty* to *many thousand* such waves each second, and yet that all these billows of air, crashing through each other from different directions at a velocity of 1120 feet a second as they approach the ear, fall undistorted against the tympanic membrane, while, *let it not be forgotten, any two systems of equal waves stand the same chance of "interference" and consequent "absolute silence" as of being heard!* No wonder that "the imagination retires baffled"!

The same difficulty applies with equal force to the Undulatory Theory of Light. The waves of *ether*—a substance which Professor Tyndall supposes to resemble a "jelly"—from a distant star, after crashing through a million other systems of *ethereal* undulations from as many stellar bodies, liable to infinitely complicated distortions, seem to enter the eye without the mark of a collision on their polished billows!

Had Professor Tyndall informed his class of scientific students how a single air-wave from E of the double bass, 28 feet long and of at least two or three feet amplitude, if symmetrically proportioned as it should be if "essentially identical" with water-waves, could make its way unbroken through a cylinder no larger than a quill, so as to make a proper impression as a *wave* on the tympanic membrane, he would have solved a problem incomparably of more importance than any sonorous demonstration made during his eight lectures, and the class could then have well afforded to let him "retire baffled" in regard to how "a thousand" such waves could all enter the ear at one time!

While these difficulties, which could be greatly increased in number, are utterly unanswerable by the wave-theory, not one of them applies with any force against the hypothesis here maintained that sound consists of corpuscular emissions radiated in sonorous discharges.

Sound, being thus an incorporeal substance, not subject to the physical laws which control air-particles or any other corporeal molecules, acts without regard to interfering objects, only as to their conductibility, just as the intangible particles of magnetism, darting from the poles of a magnet, know no interference of even the most solid and imporous substances. Yet, as shown in an earlier chapter of this work, such magnetic currents must be emanations of attenuated substance, since they actually produce corporeal effects—moving ponderable masses of iron. How simple, therefore, that sound, as constituted of corpuscular emissions, under a somewhat similar law of diffusion, should defy the interference of counteracting currents of the same substance by their passing through each other without disruption? Yet how plainly impossible is this action with air-currents when the undulations from two fans clashing in a room, with sufficient smoke admitted to visualize the air-movements, will distort and completely obliterate each other's system of waves, demonstrating that even two systems of any corporeal undulations, coming into collision, will annihilate each other and prevent all further orderly progress?

I now invite the reader to a most demonstrative argument against the wave-theory of sound, and which at the same time as conclusively demonstrates the corpuscular hypothesis to be the only satisfactory or rational solution of the problem.

I refer to the well-established scientific fact that sound is wholly unaffected by the *wind*, only so far as relates to the small effect from the *bodily movement of the atmosphere as a conducting medium*, which, in that respect, would be no different from a body of iron or water moving with or against the direction of sound while conducting it.

Contrary to the popular idea, it has been proved by the careful observations of scientific men employed in our Signal Service, as well as in the service of other nations, that fog-horns and steam sirens are many times heard against a violent gale much farther than with it, even when the atmospheric conditions seemed to be the same. This being the fact, would not the ratiocination of any reflecting mind force the conclusion that sound is something else than physical air-waves, which, so far from traveling against the wind a distance of from ten to fifteen miles, and at a velocity of over a thousand feet a second, *can not travel against it at all* even a dozen feet, when forced from the mouth of the most powerful fog-horn in the service? If the mind reasons at all from this annihilating fact so clearly arrayed against the atmospheric wave-theory, would it not at once be driven to the conclusion that sound must be some kind of corpuscular emanation which moves uninfluenced by the gross or ponderable materials through which it passes, save so far as relates to laws of conduction, somewhat analogous to those governing electricity?

General Duane, of our Signal Service, in his report to the Government, says:—

"The signal is often heard a great distance in one direction, while in another it will scarcely be audible at a distance of a mile. *This is not the effect of the wind, as the signal is frequently heard much farther against the wind than with it.* For example, the whistle on Cape Elizabeth can always be distinctly heard in Portland, a distance of nine miles, during a heavy northeast snow-storm, *the wind blowing a gale directly from Portland towara the whistle.*"

But the reader might query as to whether Professor Tyndall would be willing to admit such a fatal state of facts against his favorite theory of sound consisting simply of air-waves moulded and sent off from a fog-horn or from any other sound-producing instrument. I will allow Professor Tyndall to testify on this most essential question, as he does in his Third Edition of *Lectures on Sound*, in which he introduces a special chapter on Coast Signals. At page 296, reporting his observations off the South Foreland, he says:—

"At a distance of 9½ miles from the station the whistles and horns were plainly heard *against a wind with a force of* 4; while on the 25th, *with a favoring wind* the maximum range was only 6½ miles. *Plainly, therefore, something else than the wind must be influential in determining the range of sound.*"

"Plainly, therefore," Professor Tyndall, sound must consist of "something else than" air-waves; for if it were only atmospheric undulations, as the wave-theory so clearly teaches, it could not be heard against a wind "with a force of 4" twenty feet from the mouth of the most powerful fog-horn ever constructed. It must be an exceedingly slow wind which would not counteract the speed of air-waves sent off by the *vibrations* of a horn, which I have shown in a former argument can not reach to a distance of but a few feet in still air, while their velocity does not exceed five to ten feet a second even in a quiet room! A breeze which can be felt at all would travel faster than that.

One of the central errors of the wave-theory, and one on which its very existence hinges more completely, perhaps, than on any other, is this pivotal supposition that the vibratory motion of a sounding body, such as a string, tuning-fork, reed, or horn, acts upon the elasticity or spring quality of the atmosphere, and, by shoving its particles ahead, transmits a shock or "push" to other particles still in advance, these to others, and so on, by which means an air-wave or condensed pulse is driven off to a distance with the observed velocity of sound.

No greater mistake was ever perpetrated by physicists than to suppose such a thing as this possible with a body like our atmosphere, possessing perfect *mobility* and such trifling density, with no measurable or appreciable elasticity or spring-force, under slow displacement, *unless confined as in a tube and acted on by a piston.* I propose, therefore, at this point, to make a brief digression from this question of wind and its supposed influence on the range of sound, at least long enough to take up and analyze this problem of the so-called spring-power of the air, and with it this vital supposition of the wave-theory that the vibratory motion of a sounding body is capable of transmitting a pulse to a great distance from particle to particle of the air with the observed velocity of sound.

In the preceding chapter it was shown in different ways that there was no such a thing, in fact or in philosophy, as this so-called "spring-power," or elasticity of the atmosphere when unconfined, which would tend to transmit a pulse from particle to particle even a single foot in advance by the vibratory motion of a tuning-fork or other sounding body. Yet Professor Tyndall, in his introductory lecture on sound, teaches, in the most conspicuous manner, that the air acts in transmitting tone the same as a spiral spring, when shoved longitudinally, acts upon its own substance; and that if one particle of air should be suddenly pushed, it will communicate the push to the next particle in the same direction, it to the next, and so on, at the observed velocity of sound, and throughout the entire distance a sound may be heard, if ten miles!

To make sure that his audience did not fail to catch and retain a correct idea of this fundamental principle of the wave-theory of sound, the Professor proceeded to illustrate it, thus to impress it on the memory, by placing a row of *glass balls in a groove* so closely together as to touch each other, the end one of which being pushed longitudinally in the direction of the row would transmit the impulse through the entire line, driving off the farthest ball, just as the air-particles at a distance from a sounding body are claimed to be finally driven against the tympanic membrane, thus causing it to vibrate.

He also illustrated the same idea by employing a row of *boys*, each with his hands resting on the shoulders of the one in front throughout the line of half a dozen, more or less, the hindmost one of whom being pushed forward would communicate the impulse, by the spring-power of his rigid arms, to the next, he to the next, and so on, the last boy being pushed over, having no other boy in front of him to receive the shock! But I must quote the lecturer's words, in order to properly convey the idea:—

"I place these balls along a *groove*, thus, Fig. 1, each of them touching its neighbor. Taking one of them in my hand, I urge it against the end of the row. *The motion thus imparted to the first ball is delivered up to the second,* the motion of the second is delivered up to the third, the motion of the third is imparted to the fourth; each ball after having given up its motion returning itself to rest. The last ball only of the row flies away. *Thus is sound conveyed from particle to particle through the air. The particles which fill the cavity of the ear are finally driven against the tympanic membrane,* which is stretched across the passage leading to the brain. This membrane, which closes the 'drum' of the ear, *is thrown into vibration,*" &c.

Speaking of the row of boys, he says:—

"We could thus transmit a *push* through a row of a hundred boys, each particular boy, however, only swaying to and fro. *Thus also we send sound through the air and shake the drum of the distant ear, while each particular particle of the air concerned in the transmission of the pulse makes only a small oscillation.*"—*Lectures on Sound*, pp. 3, 5.

Now, I emphatically protest that this entire argument, from beginning to end, as thus illustrated, is the sheerest scientific nonsense, and contains not one scintilla of philosophical truth. Nothing but the manifest sincerity of the lecturer while elaborating these illustrations prevents one from suspecting that, so far from seriously intending them as a pertinent inculcation of scientific truth, he was adroitly attempting to play a practical joke on his class, or possibly might have been trying to ascertain, as a psychological experiment, to what extent an intelligent audience could be duped to believe in the most monstrous and ridiculous fallacies when inculcated as science!

To teach, as he did, that the vibrating prong of a tuning-fork moving in one direction *at the trifling velocity of only seven or eight inches in a second* (which he must have *jestingly* called "swiftly advancing"!) through a substance having the *fluxidity* and small *density* of air, should give to its particles any kind of a forward impetus or "push" which could affect the atmosphere a foot in advance of the prong, is so clearly foundationless in reason that it can only be accounted for on the supposition of a practical joke, a psychological experiment, or, if serious, as an indication of the densest innocence of all true scientific knowledge on the part of the speaker.

There evidently can be no justifiable or even pardonable excuse in a great scientist deliberately comparing this assumed *spring-power* of the free particles of air to the action of "glass balls" secured in a "groove," which must necessarily be destitute of all lateral *mobility* or power of escaping sidewise, and hence are mechanically compelled, when pushed in the manner described, to communicate their motion from the balls in the rear to those in front! Had the lecturer been illustrating the action of air *confined in a tube* and operated on by a closely fitting piston, as was done by Professor Mayer (see pages 111, 112), there would have been some appropriateness in thus exhibiting to his audience the row of glass balls restricted to a "groove." As it was, however, these balls having been employed to illustrate the spring-power of air perfectly free to move *laterally*, and to show how a body like the prong of a tuning-fork, by moving slowly through it, would shove its particles ahead, and thus transmit the "push" from one particle to another, the illustration becomes as absurd as it is unscientific and superficial.

As well might this lucid philosopher exhibit to his audience a ball of *platinum* as a pertinent illustration of the density and specific gravity of a similar ball of *cork!* Such a performance would be so flatly ridiculous that it could not be even mitigated by calling it a *joke*. Yet it would not be a whit more monstrous than to thus present the action of a row of glass balls secured in a "groove" as a suitable and pertinent illustration of unconfined air-particles circulating in free space! He might safely and pertinently exhibit the ball of platinum to elucidate the *contrast*, or point out the *difference* between it and the ball of *cork*, but not otherwise. So he could have appropriately employed the row of glass balls thus secured in a "groove" to point out the *difference* between the spring-force and elasticity of atmosphere confined in a tube, and its marvelous mobility, freedom from spring-power, and tendency to equilibrium, when circulating in open space! But really to occupy the time of his audience with the action of the row of balls, *thus secured against the possibility of lateral motion*, as a proper illustration of free air-particles, and to prove that they tend to shove each other straight ahead, as did this eminent physicist, is simply a laughable travesty on an illustrated scientific lecture; and I am astonished that any audience of sufficient intelligence to be attracted to such an exhibition could permit the speaker, however renowned, to escape scot free, and not "pin him down," to use his own words, and pulverize him on the spot, after inculcating such transparent philosophical nonsense and calling it science!

On page 112 I charged physicists with utterly ignoring the *mobility* of the air,—that is, its tendency to flow in all directions, and then form an equilibrium, whenever disturbed,—one of its most persistent and remarkable characteristics. I ask the candid reader if we have not here, in this unmistakable illustration of the row of glass balls, the clearest proof that my arraignment was just? It is entirely manifest, as any one can see, that a single word from Professor Tyndall, on the occasion of this exhibition, as to the lateral mobility of the air, or its tendency to get out of the way of a passing object by moving to the right or left, and thus take its place behind it, would have hopelessly ruined his lecture, by neutralizing every point he attempted to make out of his elaborate illustrations of the balls and the row of boys! To have taught, as he did, first that a sound is simply an air-wave transmitted as a "push" from particle to particle of the atmosphere, the same as the motion of the hindmost ball is communicated through the row, and then to have added that unlike the row of balls confined in the groove, the air-particles possess lateral mobility *and are free to slip around behind and not be pushed at all*, it must be manifest to any one would have literally shelved his whole argument, and brought down the house in laughter at such a philosophical fiasco.

He can not deny the correctness of this criticism, because, according to the clearly expressed intention of his argument as thus illustrated, and as absolutely required by the wave-theory, the air-particles in front of the tuning-fork's prong have no more tendency or power to get out of the way, to the right and left, by exercising their mobility, and thus avoid being compressed and pushed ahead, than had the glass balls confined in the "groove"! If atmospheric particles have any such a power, then away goes all this talk about transmitting condensed air-waves to a distance.

The *lateral mobility* of the atmosphere being thus wholly incompatible with that wave-motion or spring-power of the air-particles required by the current theory of sound, hence the suppression of any reference to it in the writings of physicists when discussing sonorous propagation. I assert that *not one such reference can be found in any work treating on this subject!* It speaks illy enough for the advancement of true science to have the charge justly thrust into the faces of physicists that a well-known physical fact, such as this unquestionable law of pneumatics, has to be ignored because it is in direct conflict with the pivotal and central principle of the wave-theory of sound!

Yet it stands on record, and can not be controverted, that, according to the evidence adduced all the way through the preceding pages of this monograph from the writings of these great authorities, the wave-theory of sound is continually forced to ignore the simplest laws of mechanics, pneumatics, and acoustics, in order to maintain its existence. Even if it has to assume that a trifling insect is capable of displacing and oscillating to and fro a mass of ponderable matter weighing *two thousand million tons*, as was abundantly demonstrated in the preceding chapter, this is nothing to the importance of *tympanic vibration*, for example, because that is a part of the wave-theory, and must not be suppressed! So the *mobility* of the air, exactly as self-evident as its *compressibility* or *elasticity*, must be quietly suppressed, that the ridiculous hypothesis of atmospheric spring-power in the free air may survive and be taught as a part of the current sound-theory! But ignore it as they may, physicists can rest assured that as certain as the day of doom overtakes every false theory sooner or later, just so certain does this single physical fact of the *mobility* of the air ring the death-knell of the wave-theory of sound the moment it is understood and brought to bear on the question. As well might physical philosophers attempt to ignore the fusibility of lead or undertake to suppress the law of gravitation, as to try to ward off the fatal effects of the principle of atmospheric mobility in neutralizing this so-called spring-power of the air as illustrated by the row of glass balls! This stubborn law of physics will not down at the bidding of any philosophical formula, and refuses to be suppressed or ignored any longer at the behest of any so-called scientific theory.

I do not charge these authorities with the *wilful* suppression of this scientific fact of atmospheric *mobility*. They may have done so unpremeditatedly, and I do not wish to be understood as insinuating to the contrary. Yet there is such a thing as being *scientifically dishonest* without meaning to be, or even knowing it. As paradoxical as this may seem, yet in one sense it may contain the elements of truth. Is it not possible to be so wedded to a favorite theory, and to be so in the habit of bending all our energies to its support, that in discussing its principles and the laws involved, we many times involuntarily ignore difficulties which thrust themselves in our way, and, rather than be annoyed with what we allow ourselves to fancy for the time as temporary troubles, we shut our eyes to real objections, and, by thus putting off the evil day and refusing to face them at once, absolutely ignore obstacles which, if taken up and analyzed, would have overthrown our hypothesis? Be this as it may, no man is in a condition to properly investigate the details of a scientific theory till he is able to suppress and utterly stamp out this defective tendency of human nature, and to look at physical phenomena, however they may cross his path, with the sole object of arriving at the truth, whichever way it may lead, and of accepting its principles and laws, even if his most cherished hypotheses are thereby dashed to the ground.

It is on this basis that I make my complaint and enter my charge against Professor Tyndall as a popular instructor on questions of physical science, and insist that a public lecturer so recklessly careless of accuracy, or else so blinded by the influence of a pre-adopted theory, and hence so uninformed on the scientific subjects he attempts to discuss, as not to know that the movement of the open hand through the air at a velocity of only *seven* or *eight* inches in a second could produce no effect whatever on the air-particles a foot in advance, owing to this principle of *mobility* (let alone conveying a "*condensation* and *rarefaction*" of the atmosphere to a distance of hundreds of yards, and at a velocity of over a thousand feet a second), justly earns and ought to receive the ridicule of the whole scientific world. Yet such a motion of the hand, by being continuous throughout the second, instead of being divided up into segmentary motions of *sixteenths* of an inch but of no greater velocity, ought to have more than twenty times the effect of utilizing this so-called spring-power of the air and of transmitting a condensed pulse to a distance that a tuning-fork's prong would have, being twenty times as large and passing through the air with the same velocity.

Does not every scientific thinker, who is competent to reason at all on this subject, know that if the movement of the *hand* through the air at a speed of *seven or eight inches in a second* would not send a pulse or condensation to a distance at the observed velocity of sound, then certainly the movement of the same hand *the sixteenth of an inch* in the same direction and at the same velocity could not produce any greater effect? And if the *hand* moving a *sixteenth of an inch* at that trifling velocity would produce no such condensation of the air at a distance, then pray tell us, ye astute physicists, how it is that a tuning-fork's *prong*, only one twentieth as large, moving exactly the same distance and at the same velocity, should send off an atmospheric condensation and rarefaction at a velocity of 1120 feet a second?

It was demonstrated mathematically in the preceding chapter that the prong of any tuning-fork can move only at a velocity of *seven or eight inches in a second in one direction*, and consequently that it is the essence of absurdity to suppose, as acousticians have always done, that the sound generated by a vibrating body, like a fork or string, was caused by condensed waves sent through the air by a movement of such trifling velocity. As the reader will recollect, I took the liberty of laying down for the first time the new acoustical law by which the true cause of the generation of sound was clearly expressed, to which I would again earnestly call the attention of physicists. (See pp. 92, 93.)

Nothing, in fact, but this superficial and universal misconception of supposing that a tuning-fork's prong "swiftly" advances when its movement is almost snail-like (not half as fast as a child a year old can walk, as proved at page 99), could ever have so misled physicists in regard to this erroneous idea of "moulding" and "carving" and "sending off" air-waves at the enormous velocity of sound-pulses. If it had ever once flashed across the minds of these investigators of acoustical phenomena that a sounding string or prong of a tuning-fork was never known to travel as fast as *one foot in a second in one direction*, all this nonsense about the spring-power of the free air, and of the slowly moving prong or string *carving* and *moulding* it into *condensations* and *rarefactions*, and sending them off at a velocity of 1120 feet a second by such snail-like displacement, would long since have disappeared from works on science, and physicists of to-day would be looking back with astonishment at the superficiality and stupidity of their brethren of the past, just as astronomers of the present time are often amazed at the want of perspicacity in mathematicians of the Ptolemaic school, who believed the earth to be the center of the universe, and that the sun, moon, and stars revolved around it every twenty-four hours.

As inconceivable as it must seem to the scientific students of our colleges all over the land, and as an illustration of my present argument, it is an indisputable fact that even this greatest and most reliable of modern investigators of physics, Professor Helmholtz, honestly supposed that the prong of a tuning-fork necessarily travels "*very much faster*," to use his exact words, than the ball of a swinging pendulum, as already quoted, while any scientific mechanic knows, or may know by a moment's calculation, that a pendulum having beats of two seconds each, and oscillating through a third of a circle, actually travels more than *twenty times* "*faster*" than the motion of the prong of any tuning-fork ever constructed! (See quotation from Helmholtz, page 92.)

This same investigator, looked up to as the highest standard authority on all questions of physical science in our colleges and universities, honestly supposed (because it appeared to harmonize with the requirements of the wave-theory of sound) that a violin-string oscillates normally with a velocity "*ten*" times greater than that of the bow in the player's hand, while, as it was fully demonstrated in the preceding chapter, the average velocity of the string in playing was not one fourth that of the bow, or not more than *one fortieth* as much as supposed by this world-renowned authority! (See quotation and exposition, pages 95, 96, and onward.)

Then look for one moment at the words of our most popular English authority on Sound, Light, and Heat,—Professor Tyndall,—whose works are so sought after as to be translated into most of the languages of Europe:—

"Imagine one of the prongs of the vibrating fork *swiftly advancing* [at the enormous velocity of *seven or eight inches in a second!*] It compresses the air immediately *in front* of it [Mark the language,—not to the *right* nor to the *left* of it, but "immediately *in front* of it," just as the glass balls in the "groove" push each other straight ahead if we shove the hindmost one!], and when it retreats it leaves a partial vacuum *behind*, the process being repeated at every subsequent advance and retreat. The whole function of the tuning-fork is to *carve* the air into these *condensations* and *rarefactions*." —*Lectures on Sound*, p. 62.

"*Figure clearly to your minds a harp-string vibrating to and fro; it advances, and causes the particles of air in front of it* [the same as the tuning-fork's prong, not to the *right* or *left*, but "in front," just as the glass balls and the boys push each other, straight ahead, without *lateral mobility!*] to *crowd together*, thus producing a *condensation of the air*."—*Heat as a Mode of Motion*, p. 225.

Now, it is entirely plain, if there is the slightest appropriateness in the illustration of the row of glass balls in connection with the language here used, *that sound should only travel in a line directly in advance of the moving body which generates it, since the sound is only produced by the compression of the air, and the air can only be condensed "immediately in front" of the fork or string*, just as the glass balls can only communicate their motion from one to another in the line of the "groove," no provision whatever being made for the transmission of their motion to the right hand or to the left, since all *lateral mobility* of the balls as well as of the air-particles is ignored!

Thus, the illustrations of the glass balls and the row of boys have the rare merit of consistency, being in perfect harmony with the teaching of the same authority as to the manner in which sound is sent off by a vibrating body,—namely, in *advance* only, as just quoted. In perfect keeping with this notion of *spring-power*, and according to the expressly worded language here cited, the *prong* as well as the *string* "*advances*" and "*compresses* the *air* immediately *in front* of it," and, like the balls, producing no effect either to the right or left. But when we come to consider the well-known fact that the *sound* of a tuning-fork is actually heard and equally as well at the right and left of the prong, where there is no lateral motion whatever, and consequently *where there can be no compression of the air*, what becomes of this beautiful row of glass balls and this accommodating file of performing boys? The truth is, the wave-theory of sound breaks down right here, unless logic and reason have been banished from the earth, requiring no other argument to shatter it than the illustrations and the teaching of Professor Tyndall, as just quoted; for, since the row of balls ignores the *lateral mobility* of the air, and since the prong of the tuning-fork only "compresses the air immediately in *front* of it," having no motion to the right or left, and hence no compressive force in that direction, the single well-known fact that sound is heard in that direction as well as in the line of its oscillation, demonstrates that sound is not produced by atmospheric condensations at all, and hence that this spring-power of the free air by which hypothetic sound-waves are sent to a distance is purely chimerical, having no foundation in fact.

We thus reach the unavoidable conclusion that this assumed spring-power of the free air, by which a pulse or wave may be driven off by means of a slowly moving body like the prong of a tuning-fork, amounts to absolutely *nothing*, and any physicist worthy of the name ought to know it. If I move my open hand through the air at the velocity of a tuning-fork's prong (seven or eight inches in a second), instead of the particles of air being compressed and pushed ahead on the principle of a spiral spring or in any manner analogous to the row of glass balls, thus sending a "condensation and rarefaction" off at a velocity of 1120 feet a second, any one with the faintest idea of the laws of pneumatics knows or ought to know that the air-particles in front of my hand, bringing their mobility into play, move to the right and left as the hand advances, circle around it, and in the most orderly manner take their place behind it, thus re-establishing the equilibrium and equalizing the displacement caused by the moving hand, without, in all probability, stirring the air a foot from my hand in any direction.

Did Professor Tyndall, I would ask, observe any such phenomena, while presenting these illustrations to his London audience, as the front balls slipping out of the groove to the right and left passing around and taking their place in the groove behind, as he gave the row a push? If he did not, then there was not the slightest pertinency in his illustration, or similarity to the manifest action of air-particles, since the main thing always resulting from the movement of an object such as the hand through the air, is not to cause a pulse to travel ahead to a distance or in any direction, but for the disturbed air to accomplish an equilibrium, and make good the displacement of its particles by the shortest possible route. I do not insist that an illustration shall go on all fours, or that it shall be coerced, to elucidate points not essentially involved in the argument, but I deny that there is any illustration of aerial displacement at all in this movement of these glass balls, or the semblance of analogy between the shoving of them straight ahead while confined in a "groove" and the disturbance of the freely circulating air by a slowly moving body like a tuning-fork's prong; and hence the attempt by this lecturer to represent the two actions as in the slightest degree analogous, I insist was simply practicing a deliberately contrived, though perhaps unintentional, imposition upon his audience.

Had his performing boys been half as bright as they might have been, with a few minutes' private training before they made their appearance on the platform to assist in this farcical illustration of aerial disturbance, they could have produced a genuine sensation, as discomfiting to the lecturer as it would have been beneficial to his auditors, and one which would have caused Professor Tyndall to open his eyes as they were perhaps never opened before. It would have only required the front lads of the row to gently slip out of line to the right and left and fall back to the rear as the Professor gave the hindmost boy a "push"! They would in this manner have at least conveyed some faint idea of the action of air when disturbed by a body passing through it, instead of utterly misrepresenting it, as they were forced to do under the tuition and manipulation of this great physicist! Had the boys been a dozen years old, and possessed the advantages of an ordinary education up to that age, I very much doubt, if they had been left to their own common sense, whether they might not have been able to explode this great lecture in the way intimated without any private prompting, while the audience would have evidently gone home with a good deal more of practical scientific knowledge in their heads by the trick than they received in witnessing such a worthless "comedy of errors."

But I have said enough on this question of the so-called spring-power of the air to convince, as I believe, any unbiassed mind that the small vibratory motion of a sounding body, even such as a fog-horn, would be incapable of transmitting a condensed wave to a distance of a single foot against the slightest breeze which could be felt at all, to say nothing of counteracting and traveling against a gale moving with a velocity of thirty miles an hour, or forty-four feet a second.

The sound of the fog-horn must, therefore, consist of something else than air-waves. What can it be, I ask the unprejudiced reader, if the wave-hypothesis fails to explain it, as it manifestly does? Surely there is no middle ground to assume between wave-motion and the emission of some kind of imponderable corpuscles generated by the vibratory motion of the sounding body, analogous to magnetic particles, which propagate themselves through the air and through other substances in defiance of such physical conditions as atmospheric currents.

If my hypothesis is, therefore, the true one, it would seem that this imponderable sonorous substance, whatever it may consist of, should travel at the same velocity against the wind as with it, minus the velocity of the atmosphere itself, which, being the conducting medium of the sound-particles and traveling bodily in an opposite direction must necessarily subtract that much from their speed. That is to say, if a gale is blowing twenty miles an hour, with a temperature of sixty degrees Fahrenheit, sound, which travels in still air 1120 feet a second, would move against this current but 1091 feet a second, because the air itself moves in the opposite direction 29 feet a second, which must necessarily be deducted. It is just the same in principle as if electricity traveled 1000 miles a second through a wire, while the wire was itself drawn a mile a second in the opposite direction. It requires no argument to show that the forward advance of the electric pulse would be but 999 miles a second instead of 1000. I will here venture the prediction that this formula as to the effect of wind will be found accurate whenever future science shall, by careful experiment, ascertain the facts, which will show that sound-pulses or sonorous discharges travel absolutely unaffected by air-currents, thus furnishing a clear demonstration that air-waves, with "condensations and rarefactions," and a "small excursion to and fro" of the air-particles composing the waves, have nothing whatever to do with sound-propagation, since they could not travel against the wind at all.

One would think that this fact alone, of sound traveling with nearly the same intensity and to the same distance against the wind as with it, so clearly shown by the testimony recently quoted, ought to have opened the eyes of scientific men long ago to the self-evident impossibility of sound traveling by means of atmospheric undulations driven off from a vibrating body like a fog-horn. It would really seem that a logical mind ought not to reflect on the problem one minute, in view of this evidence, without being forced to the conclusion that air-waves, with the oscillation "to and fro" of all the particles involved in the transmission, utterly fall short of explaining the phenomena of sound-propagation. That physicists have not long since reached this conclusion can only be accounted for by the fact that such a thought as calling in question the truth of the long-established and universally accepted wave-theory of sound was too preposterous a supposition to find a resting-place in their learned heads,—though they have told us over and over again, as recently quoted, that sound-waves in air move "exactly in the same way" as water-waves, are "essentially identical," and "precisely similar." Yet there is not to-day an engineer who would not laugh in the face of a man who should assert that a steamboat, anchored in a rapid stream, could send the waves from its revolving wheels as far up-stream as down, or even send them at all against the current, provided its movement was as rapid as the motion of the revolving paddles; while this same engineer, if he happened to be a disciple of Professor Tyndall, would see not the least absurdity or inconsistency in a vibrating fog-horn, which could not stir the still air over twenty-five or thirty feet from its mouth, sending *actual air-waves* against a violent gale at a velocity of a thousand feet a second and to a distance of a dozen miles with such force that the oscillating air-particles would be sensibly dashed against the tympanic membrane, causing it to physically vibrate!

It staggers human credulity that men can be found to believe such an enormous fallacy, without once calling it in question; for there is not to-day in the mythology of all heathendom a superstition involving results without adequate means more absurdly ridiculous than is the mechanical result involved in this universally accepted scientific superstition, which absolutely converts a tiny insect, as I have already demonstrated, into an engine of 50,000,000 horse-power! Yet the scientists who hold to such a monstrous impossibility, which hurls defiance into the teeth of all known laws and forces of Nature, are the very men to look with sardonic pity on a man who is so superstitious as to admit the existence of a God or to believe in the immortality of the soul!

The great diversity observed in the range of sound, when no perceptible difference exists in the state of the atmosphere, is just now the puzzling question with the scientific world, particularly with those engaged in the Signal Service of the various civilized nations.

Professor Tyndall devotes an entire chapter to this inexplicable problem, leaving it after all about where he found it, with the mere opinion that this diversity of range in clear air is due to *banks* or *clouds* of *invisible vapor* of more or less conductibility or resistance, as the case may be, to the air-waves sent off by the fog-horn! This surmise is about as satisfactory as the hypothesis of an invisible and intangible *ether* like a "jelly," filling all space and all solid bodies, by which to account for the useless undulations of light and heat, rather than admit them to be substantial emanations.

That a fog-horn or steam siren should be heard sixteen miles in a still, clear atmosphere, one day, and the next be inaudible a distance of two miles in the same direction and with the same atmospheric conditions precisely, so far as ordinary observation can determine, may well be a mystery to cause scientists to marvel, and I venture the prediction that it will never be explained satisfactorily till the true substantial nature of sound is made a factor in the investigation.

Are physicists sure they understand all about even the substantial structure of our atmosphere? Perhaps if they did, such improbable guesses as *banks* and *clouds* of *invisible vapor*, sufficiently dense to counteract *air-waves* and stop their progress, might be rendered unnecessary. Let us see if some *guess* in regard to the air itself will not more likely furnish a basis of solution for this puzzling problem than the supposition of *clouds* of *vapor* which can not be seen, yet so formidable as to stop aerial undulations!

What right have we, for example, to assume that our atmosphere is homogeneous or structureless,—the particles of which, Professor Tyndall says, swing in *ether* like suspended grains? How do we know that the molecules of the air, even in a state of rest and when comparatively free from aqueous vapor, as in a clear day, may not have been left in a relation to each other similar to that of the molecules of wood or other tangible bodies, having a lamellar structure analogous to *grain* or *fiber*, running either with the sound or at right angles to it? And how do we know but that the next current or cool night which intervenes may reconstruct these invisible strata of this wonderful substance called air, by throwing them into "pi," as the printer would say, or transversing the arrangement of their particles?

It is a well-known scientific fact that sound travels *with the grain* through certain kinds of timber, such as *fir*, with nearly six times greater velocity than crosswise of the grain, or at right angles to its exogenous rings, while it is reasonable to infer that its *range* would be correspondingly enhanced with the grain, could a sufficient body of such wood be brought together into a solid mass to test it. (See Tyndall's *Lectures on Sound*, p. 41.) This fact alone ought to have suggested the possible explanation that the same sonorous corpuscles which will select the most favorable arrangement of the molecules of wood or other solid substances for the greatest velocity or range, might also elect the most favorable arrangement of the ever-shifting air-particles, suited one day for a greater penetration than another, even when to visible appearance the conditions seem exactly the same.

If this hypothesis should be admitted (and it surely seems more reasonable than that banks of invisible *aqueous vapor* should stop the progress of sound, when it is known that *water* is a fourfold better conductor of sound than pure *air*), it at once accounts for the problem of diversity of range, with all its attendant phenomena, when atmospheric conditions appear the the same.

Take the remarkable occurrence of *echoes*, often heard returning from a clear atmosphere but a few hundred yards distant, with not a cloud in sight, and when no moisture can be detected in the air. Suppose, instead of clouds of invisible vapor (which all considerations go to render improbable), that the *grain* of the air, so to speak, or the lamellar stratification of its molecules, happen to be such as to run across the direction of the sound-discharges at a distance of a quarter of a mile from the sounding body, it would present a less penetrable surface to the sonorous pulses, and a rebound or echo would be the consequence. But the rebounding of simple *air-waves* from a bank of pure *air* is a self-evident absurdity.

There are very few persons who have not at some time or other observed that the ringing of a church or steamboat bell, the roar of a train of cars, or the noise of a cataract, would sound out with great intensity, when at other times it would be scarcely audible in the same positions. Almost universally this has been supposed to be caused by the direction of the wind, while the smallest attention shows this to be a popular mistake, since the same effect will occur exactly when there is not a breath of air stirring either way, and even when the atmosphere is comparatively free from vapor. What law, then, can explain this remarkable phenomenon so beautifully, and, at the same time, so simply, as the possible *stratification* of the air, as I have supposed? That such *grain-like texture* in the air-molecules has not been known heretofore may alone be attributed to the fact that atmosphere itself, though a corporeal substance, is invisible.

How many times, also, has it been observed, as an inexplicable mystery, by men employed in the Signal Service, and as noted by General Duane, that a sound from a siren which can not be heard at a distance of two miles in a still, clear air, can at the same time be heard distinctly six or eight miles farther on in the same direction? What hypothesis can so simply and beautifully explain this as the one here suggested? We have only to suppose that a sloping bank of air, presenting opposing *grain*, may rest on the water at a distance of two miles from the station, and that the sound-discharges, striking its slanting roof, glance over the heads of the observers, and striking another body of air with favoring grain, or with its molecular laminæ arranged longitudinally with the sound's direction, find no difficulty in penetrating it and thus reaching the ears of observers inclosed by it. The mere possibility of this explanation being the true solution, with its great simplicity, being applicable to every conceivable variety of such phenomena, most of them now regarded entirely inexplicable, would seem to commend it to favorable attention. The greatest difficulty it will have to encounter will be the mischievous idea of homogeneity wherever heterogeneity can not be distinctly traced, or where structural arrangement can not be identified under microscopical observation or by philosophical tests, which has done much to forestall explanatory investigation in more than one branch of science, as will be seen when we come to consider Professor Haeckel's evidence of spontaneous generation in the next chapter.

I do not venture the foregoing as absolutely the true explanation of the puzzling problem of diversity in the range of sound under apparently similar conditions of atmosphere, but throw it out for what it is worth, willingly trusting the science of the future to unfold a more rational solution.

In view of the facts which this single question of atmospheric currents and their influence on sound has developed, and in view of the numerous problems which seem hopelessly unsolvable by the current theory of wave-motion, may we not safely predict that a revolution is near at hand, when light, as by a new scientific revelation, shall break upon the world, and when the old hypothesis of sound-waves will be utterly abandoned by physicists for the vastly simpler and more rational view of corpuscular emanations,—against which, as the attentive reader must have observed, lies none of the immeasurable difficulties which everywhere confront the wave-theory? With this always consistent solution of every conceivable problem which the phenomena of sound can suggest made the rule of our scientific faith and practice on this question, might there not be discoveries made now undreamt of, and processes of sonorous penetration devised for piercing the densest fogs, which would not only defy the supposed stratification of the air, but all banks and clouds of vapor, visible and invisible? That such discoveries have not yet been made may be safely attributed to the erroneous basis of all our investigations on the subject of sound-transmission, or all true conceptions of even what sound is. To be wholly ignorant of the nature of sound would seem necessarily to involve very imperfect apprehensions as to its true mode of propagation or manner of conduction, as well as to the most efficient means of utilizing it to the best advantage. Truly may Professor Tyndall say, as he does in his Third Edition of *Lectures on Sound*, page 328:—

> "Assuredly *no question of science ever stood so much in need of revision as this of the transmission of sound through the atmosphere.* Slowly but surely we mastered the question; and the further we advanced the more plainly it appeared that *our reputed knowledge regarding it was wrong from beginning to end.*"

How literally yet unintentionally does this great authority express the present state of true scientific progress upon this whole question of sound, and how unwittingly has he confessed the truth when he says "that our reputed knowledge regarding it was wrong *from beginning to end*"? When he comes to realize that his own oracular words are broadly true, and that the very foundation of all knowlege on the subject—the Undulatory Theory itself—*is an* absurd fallacy "from beginning to end," he will then be able to call for "revision," with all that the term implies.

A few pages back I took occasion to animadvert somewhat severely on the first two illustrations employed in Professor Tyndall's course of lectures, namely, the row of *glass balls* in a groove, and the row of *boys*, in which he attempted to show that a body moving through the atmosphere pushes the air-particles ahead of it, or, which is the same thing, communicates motion directly ahead, as the balls and the boys communicate their motion one to another in a forward line when the hindmost ones receive a push.

I now call the reader's attention to another illustration (Fig. 4, in *Lectures on Sound*), by which the lecturer attempts to convey a similar idea, but which, if anything, is a far more signal failure than the others, because its fallacy is so clearly self-evident.

I will first briefly describe his illustration and the lesson taught by it, as shown in the engraving, which represents a tin tube fifteen feet long and two inches in diameter, having a wide flaring mouth at one end and a small conical outlet at the other for the purpose of concentrating and directing the *sound-pulse*, as he calls it, against a lighted candle-wick, thus showing how a sound-wave may be actually made to "*blow the candle out*"! He essays to demonstrate all this before his audience by placing the candle-flame directly in front of the conical outlet of the tube, and then clapping two books together at the other end, thus directing the discharge of sound or the compressed wave generated thereby into its bell-shaped mouth. The result is, the candle is, of course, *blown out;* and, on the strength of it, this accurate scientific authority declares to his audience and to the world that it is the *sound-"pulse"* and not a "*puff of air*" which produces this result! But I must quote his own literal words, or I am sure the scientific reader, if unacquainted with his book, would be tempted to doubt the accuracy of my representation:—

"At the distant end of the tube I place a lighted candle, *c*, fig. 4. When I clap my hands at this end, the flame instantly *ducks down.* It is not quite extinguished, but it is forcibly depressed. When I clap two books, B B, together, *I blow the candle out.* You may here observe, in a rough way, *the speed with which the sound-wave is propagated.* The instant I clap, the flame is extinguished; there is no sensible interval between the clap and the extinction of the flame. I do not say that the time required by the *sound to travel through this tube* is immeasurably short, but simply that the interval is too short for your senses to appreciate it. To show you that it is a *pulse* and not a *puff of air*, I fill *one end of the tube with smoke of brown paper.* On clapping the books together, *no trace of this smoke is ejected from the other end.* The *pulse has passed through both smoke and air without carrying either of them* along with it."—*Lectures on Sound*, p. 12.

As astonished as the reader no doubt is at this quotation, it is absolutely the language of Professor Tyndall, whose name is as familiar on questions of science as any household word to persons who are accustomed to reading the papers. To suppose it possible that a physicist could be found, making any pretensions as a public lecturer, who could have deliberately written out and published to the world such a statement of a scientific experiment in which he so utterly misapprehended the entire operation, passes belief, and would be scouted at once, except for the fact that we have the evidence before us in such unmistakable words that it can not be gainsaid. And it equally staggers credulity that an intelligent audience, composed largely of scientific students, could attentively listen to this lecture and not have detected the fallacious character of the doctrine taught and the misguiding tendency of the illustrations presented.

These critical students, however, looked on approvingly, and saw this eminent lecturer clap the books together in such a manner as to force the air through the tube and thus extinguish the candle, and yet never suspected the transparent nature of the deception, even after the Professor had flatly stultified himself by saying that "when I clap two books together, I *blow* the candle out"! Why did they not ask him to explain how he could "*blow*" a candle out without a "puff of air," or a "puff" of some other material substance? He might as well talk of *washing* his hands without some kind of *fluid!* Sound can not "*blow*" out a flame, or even stir it, unless it should happen to be tuned in unison, as elsewhere explained, of which the reader will soon be abundantly convinced.

This jumbling of a "sound-pulse" and a condensed air-wave together, as one and the same thing, by which the candle was *blown* out, is in exact keeping with this same lecturer's memorable solution of magazine explosions and the breaking of all the windows at Erith by a "sound-pulse," as so completely turned against the wave-theory at pages 104, 105, and onward, which the reader would do well to re-examine. Believing it possible, as does Professor Tyndall, for a "sound-pulse" to "blow" down a house, or even "blow" human beings to fragments, as has hundreds of times been done near an exploding magazine, it would have been strange indeed and flatly contradictory for him not to teach that it was a *sound-pulse* instead of a "puff of air" which *blew* out the candle when the books were clapped together at the big end of the tube! A scientific authority who was capable of believing and teaching, as he did in the same lecture, such infinite nonsense as that a church could be wrecked by a *sound-pulse,* however intense or however produced, and who was incapable of distinguishing such a pulse from a compressed air-wave, could not be expected to possess a very correct comprehension of this experiment with the tin tube, or to apprehend the true nature of the action on a lighted candle of clapping two books together. To have admitted the simple and undeniable truth that it was really a "puff of air" and nothing else which *blew* out the candle, would have been to utterly stultify all he was about to say a few pages ahead in regard to magazine explosions, since the two phenomena would have been directly opposite.

Readers of this review, if disciples of Professor Tyndall, and especially those scientific students who so quietly and approvingly listened to his lectures, will now have an abundant reason to smile at their own credulity in ever believing such a babyism as that it could have been a *sound-pulse* or anything save a "puff of air" which produced this effect of *blowing* out the candle. I ask them to give me their unbiassed attention for a single moment.

As a proof that it was "not a puff of air" which produced this result, but a "sound-pulse," look at the ocular demonstration which the lecturer had ready at hand, and which seemed to be such a clincher as to silence and literally overwhelm any scientific doubting Thomas who might happen to be in the assembly! "I fill *one end* of the tube with the "*smoke of brown paper*"! Which "end," Professor? Why, of course he was too shrewd and skilled a public lecturer and experimenter to fill the *wrong end* of the tube, or the one nearest to the candle, for he well knew (or if he did not know it he is to be pitied) that if he had filled the small end with smoke, instead of the large end fifteen feet away, *a visible "puff" would have greeted his audience every time the books came together*, and would thus have ingloriously exploded the whole deception! Hence, he was cautious enough to put the smoke into the large end of the tube, so that it would be compelled to travel *fifteen feet* before it could pass out at the small end, which would have required at least five or six powerful *claps* of the books to carry it that distance! Of course this was purely accidental, as we must charitably suppose, since it never occurred to this able and authoritative investigator of science to fill the *entire tube* "with the smoke of brown paper," and then see whether it would "puff," which would have been more easily done than filling "one end" of it, because special care had to be used not to let the smoke creep ahead too far into the tube, or too near to the outlet, lest an accidental "puff" should undeceive the audience,—while this critical class of scientific students equally forgot to request him to do so! They constituted, to say the least, an audience remarkable for deference to authority if not for scientific perspicacity, and proved themselves unprecedented for the marvelous character of their amiability,—literally sitting there and taking down the logic as well as "smoke of brown paper," without asking a question or offering the least interruption except to applaud!

It is true it seemed impossible to suspect a trick of prestidigitation or anything wrong on such an occasion, especially from the apparently frank and candid style of the lecturer. He did not hesitate to tell his auditors, in the plainest language, that it was "*one* end of the tube" only which he filled "with the smoke of brown paper," and they saw distinctly, when he put the lighted brown paper into it, which "end" of the tube he meant; so there was apparently nothing unfair or disingenuous in the performance. Then, after filling this particular "end of the tube," he honestly clapped the books together in front of the bell-shaped mouth, without "a trace of this smoke" being "ejected from *the other end*"! After such a conclusive demonstration, is it any wonder that he should have so triumphantly added: "*the pulse has passed through both smoke and air without carrying either of them along with it*"?

But now I ask, seriously, how did Professor Tyndall know that no *air* was carried out of the small end of the tube when he clapped the books? Evidently in the same way exactly in which he knew that no *smoke* was carried out,—*he did not see it!* The reason why he did not see the *smoke* pass out was because it could not get out, since it was impossible for it to travel the whole length of the tube at a *single clap!* This, to say the least, was a good and sufficient reason. Smoke being a *visible* substance, it was absolutely essential to the success of the experiment that it should not pass out when the books were clapped, or it evidently would have been seen by the audience. Hence, as before stated, that was mechanically provided against by placing the lighted brown paper in the proper end of the tube fifteen feet away from its outlet. But the air being entirely *invisible*, it made no difference if the tube was full of it, as it necessarily was, and it mattered not a whit if the air puffed out at the small end every time the books came together, as it manifestly did, it was the easiest thing in the world for this eminent lecturer to assume and announce to his audience that "the pulse has passed through both smoke and *air* without carrying *either of them* along with it," because he knew very well that the most argus-eyed scientific student present could not see a "puff of air" even if it did pass out!

Here, again, we have this same *invisible* dodge which was so convenient in discussing the *amplitude* of sound-waves, in which the air-particles were claimed to oscillate "to and fro with the motions of pendulums," and as having "comparatively a *large amplitude of vibration*," yet which turned out to be no amplitude at all—not even enough to be seen by the aid of a microscope—when brought to bear on iron with waves admitted to be *seventeen times as long!* Air being wholly *invisible*, these physicists seem to claim the right of assuming anything in regard to it which happened at the time to suit their theory, appearing to feel safe against adverse criticism, since no one can see a "puff of air," and therefore, as they suppose, dare not contradict them!

But I have concluded that this *invisible* dodge shall end here and now. It has been played by these learned investigators of science and imposed upon a credulous world just about long enough. I here undertake to suggest a few practical scientific tests in connection with this experiment of the tin tube, each one of which is worth a thousand such shallow legerdemain tricks as filling "*one end* of the tube with the smoke of brown paper,"—tests which any student can at once demonstrate for himself who is at all interested in ascertaining the truth or falsity of the wave-theory of sound, or who may care to know the exact scientific weight of Professor Tyndall's authoritative statements, even on simple questions of fact. These experimental tests are as follows:—

1.—Take a common paper bag, such as grocers use for putting up packages, having the air completely pressed out of it, and, after tying its mouth closely around the small end of the tube, proceed to clap the books at the large end as described by Professor Tyndall, and I pledge my scientific veracity and all the reputation I ever expect to have, that the first clap will partly fill the bag, and that it will be distended more and more at each succeeding clap till it is entirely filled and rounded out with air!

This high authority on science, whose achievements are in every one's mouth, assures his audience that no air is "ejected from the other end" of the tube,—nothing at all, in fact, but sound, since "the *pulse* has passed through both *smoke* and *air* without carrying *either of them* along with it." Hence, we have the astonishing phenomenon of a paper bag *stuffed full of sound*, which can be transported from place to place like so much sugar or salt!

Who will dare hereafter to look upon Munchausen's story of the frozen horn as an improbable narrative, with its music thawing out in melodious strains hours after it had been congealed while the bugler was blowing it? It may turn out to be no acoustical joke, as generally supposed, if there is the least truth in the foregoing description of the "scientific" experiments of this eminent investigator, whose discoveries in connection with a simple *tin tube* utterly distance the *telephone* and its lineal descendant the *phonograph;* for these only claim to transmit by electricity the *motions* which generate the sound, and then preserve their impressions on foil, by which they can be repeated in the same manner, and, if desired, at a future time,—while Professor Tyndall's great improvement *actually bags up the tone itself*, like dessicated fruits, in pint or quart packages, ready for use! There is no mistake about this startling deduction; for whatever passes through the tube, on clapping the books together, fills the paper bag, whether it be *air*, *smoke*, or *sound;* and as Professor Tyndall, with the whole force of his great reputation as a scientist, has published to the world that it is nothing but *sound* which passes out of the tube, hence the undeniable correctness of the criticism.

2.—Place the lighted candle at the small end of the tube, as described by the lecturer, and, instead of clapping the books together toward the bell-shaped mouth in such a manner as to drive the compressed wave into it, let the books be held sidewise toward the expanded entrance, and, although they may be clapped with ten times the force and produce a sound ten times as loud, this learned physicist will find to his confusion that it will neither "blow the candle out" nor make it "duck," simply because in this position it drives no "puff of air" through the tube, notwithstanding the actual sound passing through it may have ten times the intensity as when the candle was extinguished. It does not require a scientific reader to see that this single fact completely annihilates Professor Tyndall's whole argument based on this experiment of a tin tube, and with it the wave-theory of sound, which, in every one of its phases, is in perfect keeping with this experiment, so transparently absurd that even a stupid schoolboy ought to be ashamed to make it.

3.—Vary the test by leaving the candle as before, and instead of clapping the *naked* books together so as to cause a *report*, let their sides be *cushioned*,—or, rather, which is better, let them be prevented from coming entirely together by an intervening piece of soft rubber, and although no audible sound will be produced, yet such a noiseless "clap" will "blow the candle out" exactly the same as in the former case, where the clapping of the books generated a sharp report, and for the same reason, namely, that it was not the sound at all which extinguished the flame, but the "puff of air" which will pass through the tube with precisely the same facility when books are cushioned and noiseless as when they are naked and produce a sharp sound. Yet this renowned lecturer, notwithstanding all his reputed scientific skill, could think of none of these simple and practical tests, by which to have so easily demolished his illustration of the tin tube and lighted candle, and by which he had so cleverly, though perhaps unintentionally, deceived the public. I say perhaps *unintentionally*, because I am not yet ready to believe that this lecturer knew any better, but rather that he actually supposed that it was a *sound-pulse* and "not a puff of air" which *blew out the candle*.

For my own part, however, I would about as soon have the reputation of being a little tricky in my public experiments on scientific questions as to prove myself so superficially innocent of all practical or theoretical knowledge of the simplest laws of mechanics, pneumatics, and acoustics, while attempting to instruct the public. It seems strange, to say the least, that a physicist who was so *ingenious*, if not *ingenuous*, as to put "smoke of brown paper" into "*one end of the tube*," and to make sure that this end was the one fifteen feet away from the outlet, ought to have possessed sufficient originality to have thought of some one of the practical tests just named,—either one of which, if fairly made, would have utterly exploded that tin tube experiment, and with it the entire wave-theory of sound, because the principle involved in this experiment—that a condensed *air-wave* and *sound-pulse* are one and the same thing—lies at the very foundation of the current hypothesis, as every well-informed scientific student knows.

4.—And lastly, if our eminent physicist was really honest in his experiments (which common charity compels us to assume till the contrary is demonstrated), and did not know any better than to make such a careful blunder with the "smoke of brown paper," he has now an excellent opportunity, by a final and simple test which I will name, of not only informing himself on these fundamental questions of physical science, but of placing himself right upon the record by publishing to the world a correction of his book on "Sound," and thus undoing to the extent of his ability the mischief he has already wrought in so grossly misleading the public.

On reading this friendly criticism (for I assure him that these animadversions are entirely friendly, though necessarily severe), let him at once bring out his apparatus employed on the occasion of those lectures, and instead of filling "*one end* of the tube with the smoke of brown paper," let him fill the *whole tube*, and then proceed to clap the books together the same as he did to "blow the candle out," *and if he does not see a puff of smoke "ejected from the other end" every time the books come together*, he has the fullest permission to publish the author of the *Evolution of Sound* to the world as the great anonymous North American falsifier and slanderer, and all the people shall say "Amen!"

Should even this test not prove entirely satisfactory to the Professor that his whole experiment was a baseless and superficial mistake, after he has witnessed, as he will, the ejection of a dozen separate puffs of smoke, let him fill the tube with the fumes of burning sulphur, and then place his nose in the exact position previously occupied by the candle while his assistant claps the books, and I undertake to guarantee that after the first clap he will become a convert to the new theory, and get away as soon as possible, with a well-defined conviction, which will be apt to stay by him as long as he lives, that something besides *sound* passes out of the tube on clapping the books!

In view of the undeniable correctness

of the four or five tests here suggested, I now appeal to the logical intelligence of the readers of this monograph, if it is possible for a theory to be based on scientific principles which ignores such simple truths, and which is continually, as seen during the course of this discussion, forced to resort to such transparent fallacies as the experiments under examination.

Is it at all likely, or even conceivable, that a true scientific theory would have to depend for its existence on the most superficial and contradictory errors, the jumbling together of the most self-evident unanalogous effects and making them one and the same thing, as has been so clearly and repeatedly pointed out from the commencement of this review? How it is possible for a physicist to acquire such a world-wide fame, whose scientific writings from beginning to end are filled with just such self-contradictions, puerilities, and practical absurdities, as those here being exposed, defies the powers of human imagination to conceive.

While I freely admit that many of the illustrations presented in Professor Tyndall's book on "Sound" represent phases of sonorous phenomena on which there can be no controversy, such as the ringing of a bell in vacuo which gives off no sound, the vibratory motion of strings, the reflection and convergence of sound, the action of singing flames, &c.,—showing clear conceptions of the problems discussed, yet it may be safely asserted that not one single illustration can be pointed to which directly involves the truth or falsity of the wave-hypothesis which can not be shown to be based on a pure misconception of the principles and laws of mechanics, acoustics, and pneumatics, involved. I fancy the attentive reader of this treatise has already seen enough to create at least a strong pre-*sumption* in his mind that there may be a good deal of truth in this general arraignment of the theory, as well as its most popular exponent; at all events, sufficient to warrant a careful examination of what is to follow.

Not to make this discussion too extended, I shall undertake to examine only the very strongest points made by Professor Tyndall during this course of lectures in favor of the current hypothesis, knowing, as the reader must, that if the arguments deemed most conclusive fall to the ground, the weaker ones do not require refutation.

I now call attention to an experiment made, apparently, for the express purpose of demonstrating the truth of the wave-theory, and which, if based on a truthful representation of facts, would have been most difficult to explain except in conformity with that hypothesis. I may add that to a superficial reader it would perhaps come nearer what might be called demonstrative evidence than any other illustration in the book. But the facts being entirely misapprehended by the lecturer, as I proceed to show, the argument built upon them must necessarily break down on simply correcting the facts.

To prepare the reader for this experiment, I will state that it is known to every student of acoustics that a tuning-fork, when sounded over the mouth of a jar, having a depth corresponding exactly to its own pitch or vibrational number, will produce a loud and very pure sound, caused by the resonance of the column of air vibrating in unison with the sounding fork; whereas the slightest increase or decrease in the depth of this column, by pouring out or adding water, will correspondingly diminish this resonance, or destroy it entirely if the variation from exact resonant depth be carried to any considerable extent.

Professor Tyndall made this experiment before his audience with a tuning-fork having 256 vibrations in a second, and a consequent wave-length, according to the current theory, of 52 inches from condensation to condensation,—that is, supposing the velocity of sound to be 1120 feet in a second, as it is at a temperature of about 60 degrees Fahrenheit.

The lecturer held the sounding fork over the jar in the usual way, while gently pouring in water from a pitcher till the column of air had reached the exact resonant depth corresponding to the pitch of the fork, when the sudden outburst of tone warned him to desist. And right at this point comes in the supposed conclusive argument in favor of the wave-theory of sound. With a two-foot rule he measured the depth of this chamber in the presence of his audience, and declared it to be 13 inches, or exactly one quarter of the wave-length from a fork of that pitch, or having that number of vibrations per second.

Of course this was, to say the least, a singular and even surprising coincidence, on any other supposition than the truth of the wave-theory. But his explanation of the matter made the remarkable character of the coincidence still stronger. He explained the problem in this wise: The *condensation* of the sound-wave sent off from the fork passes down to the water and back (26 inches) in half a second, succeeded by the *rarefaction*, which makes the same round trip in the same time, thus making the complete wave-length of 52 inches in a second, as it ought to be according to the requirements of the theory.

Under the circumstances, I can not blame his auditors for applauding this beautiful experiment, as it was not possible for them to detect any trick or misrepresentation of facts, seated in the auditorium, as was so clearly apparent, and ought to have been detected even by a schoolboy, with the illustration of the "tin tube" and "smoke of brown paper," just examined. Without having practically gone over this somewhat complex experiment with the suitable apparatus, no one would have been inclined to doubt the actual results as given by Professor Tyndall, especially with prejudices already in favor of the current hypothesis of sound. I am not therefore surprised that the lecturer succeeded in completely deceiving his auditors (whether intentionally or unintentionally the reader shall decide), and sending them away satisfied with the truth of the wave-theory. But a day of reckoning has to come sooner or later for all our errors, whether sins of commission or omission. The learned physicist has no more right to expect immunity from a just retribution than the most ignorant pretender and upstart in science; and, in fact, not so much, since to whom much is given of him shall much be required.

Before undertaking to expose the fallacy of this illustrated argument, I must, as usual, and in justice both to myself and to Professor Tyndall, quote his exact words, or at least make a sufficient citation to convey his meaning in his own very clear and explicit language:—

"A series of tuning-forks stands before you, whose rates of vibration have been determined by the siren. This one, you will remember, vibrates 256 times in a second, the length of the sonorous wave which it produces being, therefore, 4 feet 4 inches. The fork is now detached from its case, so that when struck against its pad you hardly hear it. I hold the vibrating fork over this glass jar, A B, fig. 87, 18 inches deep; but you still fail to hear the sound of the fork. Preserving the fork in its position, I pour water with the least possible noise into the jar. The column of air underneath the fork becomes shorter as the water rises. The sound, you observe, augments in intensity; and when the water reaches a certain level it bursts forth with extraordinary power. . . . Experimenting thus I learn that there is one particular length of the column of air which, when the fork is placed above it, produces a maximum augmentation of the sound. This re-enforcement of the sound is named *resonance*. . . . Our next question is, *what is the length of the column of air which most powerfully resounds to this fork?* By measurement with a *two-foot rule* I find it to be *thirteen inches*. But the length of the *wave* emitted by the fork is 52 inches; hence, the length of the column of air which resounds to the fork *is equal to one fourth of the length of the wave produced by the fork. This rule is general, and might be illustrated by any other of the forks instead of this one.*" —*Lectures on Sound*, p. 172.

To satisfy myself as to the exact facts in regard to this experiment, and to be certain that my statements in review should be correct, I obtained from Professor Robert Spice, the eminent acoustician of Brooklyn, N.Y., an accurately tuned, tested, and stamped tuning-fork, having exactly 256 vibrations in a second, that there should be no possible error committed in overhauling this celebrated experiment and the argument deduced from it, as published to the world by Professor Tyndall.

Thus equipped, I proceeded to test a glass jar, straight from bottom to top, by pouring in water while the fork was sounding over it, as was done by Professor Tyndall, till the greatest resonant depth was obtained. I now declare, after testing a number of different jars of various diameters, from four to two inches (which, by the way, give a uniform result), that the length of column or greatest resonant depth for such a fork, at about 60 degrees Fahrenheit, is invariably $11\frac{3}{4}$ inches instead of 13, as stated by this "highest living authority," thus making the wave-length 47 inches instead of 52, as it should be according to the wave-theory! With 47 inches as the wave-length, multiplied by the number of vibrations (256), we would make the velocity of sound but 1002 feet in a second, at 60 degrees Fahrenheit, instead of the observed and well-known velocity of 1120 feet a second! Thus the wave-theory is overthrown by the very argument adduced to sustain it, while the reader undoubtedly asks how could it be possible for Professor Tyndall to perpetrate such a glaring mistake, with the glass jar before him, and with a proper tuning-fork and a correct "two-foot rule" in his hand! The error, as we see, is a fatal one, since it makes a positive difference of 118 feet a second, as any tyro in mathematics can instantly determine, between the observed velocity of sound and what it is forced to be according to the formula of Professor Tyndall, in trying to sustain an untenable and foundationless theory.

But I will now try to relieve the mind of the reader, and tell him in unmistakable words how this mistake occurred in Professor Tyndall's calculation; and also, I may add, in the calculation of Professor Helmholtz, who agrees with Professor Tyndall fully that the greatest resonant depth of a jar is *one quarter of the wave-length of the determinate tone thus augmented;* so that these two great physicists fall, as usual, side by side, whenever one is tripped.

Those having access to a copy of the *Lectures on Sound* will observe that the engraving represents a jar having an *expanded or bell-shaped mouth!* This single fact is the key which unlocks the mystery and solves the whole problem, giving the true reason for Professor Tyndall's trouble in a nutshell. In order to demonstrate the correctness of this solution of the difficulty, I had three jars made specially for this experiment, all of the same diameter and height,—one straight from bottom to top, one with an expanding mouth, the expansion being about one half the diameter of the jar and extending down a couple of inches, and the third with the mouth contracted or drawn in about as much and about in the same proportion as the other was expanded.

By means of a series of careful tests with the same fork—256 vibrations to the second—I found that while the straight jar gave invariably a resonant depth of 11¾ inches, the one with the bell-shaped mouth gave a depth of 12¼ inches, while the one with a contracted mouth gave a depth of but 11½ inches. The conclusion was thus scientifically reached that with the mouth of the jar sufficiently expanded, and carrying the expansion a sufficient distance down, a resonant depth of exactly 13 inches might be finally attained, and in this way the experiment could be made to precisely harmonize with the necessities of the wave-theory, making 52 inches the wave-length instead of 47,—as results, and must always result, from using an honest jar!

It is not at all likely that this lecturer, in the presence of an intelligent audience of scientific men, would have stated that the resonant depth of this jar was *thirteen inches*, by actual measurement with a "two-foot rule," when it was but *eleven inches and three quarters!* And it would not be fair to suppose that he had a bogus "two-foot rule," or that he was capable of playing any such "tricks that are vain" as running the rule up his sleeve while making the measurement! We are bound, therefore, to admit that his measurement was honest, and that the jar showed an actual resonant depth of 13 inches; but, at the same time, we are driven to assume that the mouth of the jar *flared*, as his engraving indicates, just enough to make up this deficit of 1¼ inches, thus to sustain the wave-theory!

Now, I do not intend to insinuate that there was any conspiracy between the Professor and his glass jar by which its mouth was to *flare* just enough and not a whit too much to make up these thirteen inches of resonant depth! As a supposition so flagrantly unkind is out of the question, it becomes one of the most remarkable coincidences known to science that such a long glass vessel should be blown with a mouth flaring just enough to answer the purposes of this theory, and that it should have occurred fortuitously, or without pre-calculation, design, or intention, on the part of anybody! A man who could believe this would require but little stretch of his credulity to believe, with Mr. Darwin, that man, with all his complicated powers, might have been accidentally developed by a series of fortunate spontaneous variations to what he now is, from a horned toad or a soft-shell clam.

The serious part of the whole matter, however, viewed from a scientific standpoint, seems to be this: Even supposing that particular jar, having just such a flaring mouth, should have fallen into the lecturer's hands accidentally on that particular occasion, which so luckily hit the nail on the head and demonstrated the truth of the wave-theory, is it conceivable that this great sound-expert and experimenter, who had devoted much of his life to the investigations of sonorous phenomena, including this same beautiful problem of resonance, never happened at any other time to try this experiment with a *straight* jar, or, in fact, with any jar not flared exactly to that extent? If he ever held a tuning-fork of any determinate pitch over a *straight jar*, and then brought into requisition his "two-foot rule," *he certainly must have seen that the resonant depth thus resulting was considerably less than the one quarter of a wave-length of the particular fork employed!*

To meet the difficulty, and rescue this eminent lecturer from the fatal effects of his own argument, we are forced to assume that in all his experience he never used but the one jar, having that particular *flare* to its mouth, and never saw such an experiment tried by any one else as holding a tuning-fork of a determinate pitch over a *straight* jar from bottom to top, or over any other jar having a bell-shaped mouth differing in the slightest degree from the one which so fortunately fell into his hands for that special occasion!

Whatever explanation may be attempted of these singular and uncomfortable facts, and however this lecturer may essay to rescue his experiment from the suspicion in the mind of the reader of a conspiracy between *somebody* and that particular *glass jar*, one thing is settled beyond all possible doubt by the unfortunate dilemma in which this eminent physicist has involved himself, which is this: the wave-theory of sound has fairly and utterly broken down, judged alone by the strongest argument ever employed to sustain it, since the theory's own explanation of the supposed wave-length contradicts the observed velocity of sound, when an honest jar is used, by just 118 feet a second! Oh, for some modern Laplace to help Professor Tyndall out of his difficulty by a new formula of heat and cold—condensation and rarefaction—to account for this discrepancy of 118 feet a second, as the original Laplace so triumphantly succeeded in *not* doing it with the deficit of 174 feet a second discovered by Sir Isaac Newton!

The next illustrated argument in this course of lectures on sound, to which I would invite the attention of the reader, is perhaps the most astonishing for pure baselessness ever presented in favor of a scientific theory, being particularly remarkable for two things: the first, that it is advanced as a specially conclusive evidence in favor of atmospheric wave-motion (which it certainly would be if true); while in the second place, there is not the semblance of scientific truth in even the assumed facts on which the whole argument is based. The correctness of this apparently exaggerated assertion will be abundantly evident to the reader as the analysis of the position advances.

I have pondered frequently over the argument to which I now refer, and every time with undiminished amazement to think that a careful physicist and competent investigator of scientific phenomena should have been so presumptuous as to imagine it possible for a person, claiming to reason at all, to accept the pretended facts so deliberately assumed and specifically paraded. At times I confess to having been inclined to half suspect my own want of perspicacity in not catching the true meaning of the text, it seeming so entirely inconceivable that a person, pretending to even ordinary scientific knowledge, should have assumed as facts, simply because a theory happened to require it, what a very stupid schoolboy a dozen years old could readily have seen to be without a shadow of foundation;—facts as preposterously and transparently out of the question as if he had stated to his audience that the *swaying shadow of a tree had weight and momentum sufficient to knock a man down should he come in contact with it!* But after discussing the matter and comparing views with others,—even believers in Professor Tyndall's theory of wave-motion,—and finding that the most critical scientific thinkers were obliged to place the same construction on his language that I had done, there was nothing left but to accept his literal statement of assumed scientific facts, and then meet his extraordinary argument. With these preliminary remarks, I will now, as usual, proceed to briefly state the argument before giving the exact words of the lecturer, that the reader may know what specific point to expect.

As is well known to every scientific student, and as previously shown by quotations, the wave-theory assumes that two systems of sound-waves, from two unison instruments, traveling through the same air together, may so travel as to assist each other or augment each other's sound; that is, when they travel in such a manner that the *condensations* of one system of waves coincide with the *condensations* of the other system, and the *rarefactions* of the one with the *rarefactions* of the other, the same as two systems of water-waves will make higher billows when they travel together in such manner that the *crests* of one system coincide with the *crests* of the other, and the *furrows* of the one with the *furrows* of the other.

It is also well known that if two equal systems of water-waves travel together in such manner that the *crests* of one system coincide with or fall into the *furrows* of the other system, they will mutually destroy or neutralize each other, producing a level, or nearly so. This is called *interference*. But as atmospheric sound-waves are claimed to be "essentially identical" with and "precisely similar" to water-waves, hence it seemed unavoidable, as a vital feature of the wave-theory, that physicists should teach, just as they do, that if two unison systems of sound-waves should happen to travel in such relation that the *condensations* of one system should coalesce with or fall into the *rarefactions* of the other system, they must necessarily neutralize each other or produce *absolute silence*.

As I saw that this was the evident and unavoidable reasoning of physicists, I undertook, when first investigating the wave-theory, to expose its fallacy by showing that if it were so, then two unison pipes, forks, or reeds, sounded *half a wave-length apart*, could not be heard at all by a listener stationed in the line of the instruments, because in that direction the two systems of waves would be compelled to travel in complete *interference*, the *crests* or *condensations* of one system matching into the *furrows* or *rarefactions* of the other, thus producing a *level*, or neutralizing each other's effect; whereas, if the instruments were sounded a *whole wave-length apart*, then their united sound would necessarily be much louder in the line of the instruments than either would be alone, because the two systems of air-waves would reenforce each other by coincidence,—their *condensations* would run together as well as their *rarefactions*, and thus augment each other's effect on the air the same as shown in water-waves.

Of course I supposed that I was advancing a *new* argument against the theory, and one so self-evidently fatal to it, being the unavoidable consequence or natural outgrowth of this "law" of *interference* that the moment physicists would see it they would necessarily be compelled to abandon the wave-hypothesis as a self-stultifying absurdity, since such an idea as two unison instruments not being heard when sounded in line, whatever distance apart, whether a half or a whole wave-length, was so transcendently absurd and contrary to all observation and reason that I did not consider it necessary to more than state the fact in order to annihilate the assumption of atmospheric sound-waves! I never dreamt of such a thing as that physicists had thought of the same argument, much less that they had appropriated and adopted it as a part of their system. The reader can guess my astonishment to find, in carefully reading Professor Tyndall's *Lectures on Sound*, that my own crushing argument against the wave-theory had been clearly anticipated and coolly presented to his audience as an illustration of this very law of *interference*, and the manner in which sound can be so added to sound as to produce silence!

Thus, we come at last to the argument to which my preliminary remarks had reference. In elucidating this law of "interference" in his book, Professor Tyndall has presented engravings representing two unison tuning-forks placed first a wave-length and then half a wave-length apart. Suppose each of the two forks to have exactly 256 vibrations in a second, and a consequent wave-length of 52 inches, he shows by the most careful explanation that if the two forks should be placed 26 inches apart (half a wave-length), and be then made to vibrate ever so vigorously, no sound would be heard in the line of the two instruments, which is illustrated in the engraving by a smooth and uniform shading passing off from the forks, thus representing the quiescent condition of the air. He also shows by the other figure that if the two forks are placed 52 inches (a whole wave-length) apart, the sound will be distinctly heard in line, the waves of which he represents by alternate dark and light shadings passing off from the forks in the same manner, thus teaching that any two unison musical instruments, however intense their tone may be, if thus sounded half a wave-length apart, would neutralize each other, and not be heard at all in the line of such sounding bodies.

With this explanation before the reader, I will now quote Professor Tyndall's own words, to show that it is not a misconception of his meaning:—

"Now let us ask what must be the *distance between the prongs A and B* [one prong of each of the two forks] when the *condensations* and *rarefactions* of both, indicated respectively by the dark and light shading, *coincide?* A little reflection will make it clear that *if the distance from B to A be equal to the length of a whole sonorous wave* [52 inches] *coincidence between the two systems of waves must follow.* The same would evidently occur where the distance between A and B is two wave-lengths, three wave-lengths, four wave-lengths,—in short, *any number of whole wave-lengths.* In all such cases we should have *coincidence* of the two systems of waves, and consequently a reinforcement of the sound of one fork by that of the other. . . . But if the prong B *be only half the length of a wave behind A* [26 inches] what must occur? *Manifestly* the *rarefactions* of *one of the systems of waves will then coincide* with the *condensations* of the other system, and we shall have *interference;* the *air* to the right of A *being reduced to quiescence.*"—*Lectures on Sound*, p. 259.

Before commenting on the above citation, which distinctly teaches what I have asserted, I wish to guard against the remotest suspicion of misconceiving the Professor's meaning of "condensation," "rarefaction," "coincidence," "interference," &c. It is of the highest importance, also, that the reader shall know from the lecturer's own words that I have not misapprehended him in the slightest degree. To this end I now quote a passage which leaves no possible doubt. He says:—

"In the case of *water*, when the *crests* of *one system of waves coincide* with the *crests* of *another system*, higher waves will be the result of the coalescence of the two systems. But when the *crests* of one system coincide with the *sinuses* or *furrows* of the other system, the two systems in whole or in part *destroy each other.* [Of course, no one doubts the truth of this statement as applied to water-waves, because there we have actual wave-motion.] This *mutual destruction* of two systems of waves is called *interference.* The same remarks apply to *sonorous waves.* If in *two systems of sonorous waves condensation coincides with condensation* and *rarefaction with rarefaction*, the sound produced by such *coincidence* is *louder* than that produced by either system taken singly. But if the *condensations* of the one system *coincide* with the *rarefactions* of the other, a *destruction* total or partial of *both systems is the consequence.* . . . If the two sounds be of the *same intensity* their *coincidence* produces a sound of four times the intensity of either; *while their interference produces absolute silence.*"—*Lectures on Sound*, pp. 284, 285.

This language can not be misunderstood. Two equally intense systems of sound-waves from two unison instruments, placed ***half a wave-length*** apart so that their waves "interfere," must of necessity destroy or neutralize each other, and thus produce "***absolute silence***" ***either way in the line of such instruments***, if there is any truth in this pretended law of "***interference.***"

It must, therefore, be entirely plain to the reader, if the wave-theory be true, and if any such phenomena as atmospheric ***sound-waves*** do actually occur in sonorous propagation, having ***condensations*** and ***rarefactions***, ***amplitude*** and ***wave-length*** in feet and inches, that this law of "***interference***" must also inevitably follow, just as physicists have represented it, for such is indisputably the law which prevails in water-waves, where we know that a veritable ***amplitude*** and ***wave-length*** exist. Hence, to have ignored this law of "***interference***" in sound would have been to ignore sound-waves altogether; and therefore, as was naturally to be expected, Professor Tyndall teaches undisguised "***interference,***" with its resultant "neutralization" or "***absolute silence,***" in the manner here quoted.

But just as true as "interference" is a necessary law growing out of wave-motion, whether in ***air*** or in ***water***, just that certain is it that the whole wave-theory falls to the ground whenever this law of sonorous "interference" is shown to be without foundation in fact. I now undertake to assert that such a law, in relation to sound-propagation, is purely visionary and monstrously chimerical, having no existence in Nature, and not even the appearance of a properly understood fact to warrant it. Strange as this may sound to physicists, they will be more than satisfied of its correctness before this chapter is finished.

As one evidence that the law is without foundation in science or in fact, we need no better proof than the test here distinctly prescribed by this lecturer himself, namely, the placing of two unison instruments half a wave-length apart, and then sounding them with listeners stationed in line either way to determine by actual observation the truth or falsity of the principle enunciated. Professor Tyndall distinctly tells us that two such instruments would not be heard in line, however loudly they might sound or however distinctly one alone could be heard if the other was silenced. It would really seem that an intelligent reader need scarcely be informed that there is not one scintilla of scientific truth in this whole statement; and how a physicist, having any regard for accuracy or the just respect of the scientific world, could have published such a fabrication as part of a scientific lecture, to meet the necessities of any theory, however firmly established, is more than I can imagine. That the wave-theory requires such a "law" of interference as well as such practical fruits in the form of "neutralization" and "absolute silence" there can be no question. In fact, its very life depends upon the truth of Professor Tyndall's statement, or otherwise, as just shown, there can be no such thing as sound-waves at all, and the whole wave-theory consequently breaks down. Believing, as did this eminent scientist, that the wave-theory could not be otherwise than true, and knowing that if true, the law of "interference" and its effect of "absolute silence" must follow, as a matter of course, with two unison instruments sounding half a wave-length apart, hence he seemingly shut his eyes to the necessity of testing the matter, and ran headlong into this ridiculous position, which a schoolboy with two penny whistles of the same pitch and a couple of babies for assistants, could instantly have shown to be without a particle of foundation in truth!

As a final and unanswerable experiment

for the purpose of testing this supposed law of "interference," on which, of course, the existence of the wave-theory depends, the reader has only to figure before his mind's eye two immense organ-pipes of equal capacity which sound the low E of the double bass, having each 40 vibrations to the second, and a consequent wave-length in air of exactly 28 feet. Then figure these two pipes placed precisely 14 feet apart in an open field, free from any reflecting surfaces, each pipe supplied with wind from a powerful bellows, and the witnesses stationed on either side in line with the pipes. It is manifestly evident when these pipes are sounded in this position that their two systems of unison waves (if they produce waves at all, or if the wave-theory has any foundation,) will travel in the direction of this line in absolute "interference"; that is to say, the *condensations* of the waves from one pipe will exactly coincide with, or fall into, the *rarefactions* of the waves from the other, and hence along that line the witnesses would hear no tone if this law of "interference" has any existence in sound, while another jury of witnesses placed to the right and left, equidistant from the two pipes, would hear their united sounds *with four times the intensity of either pipe sounded singly!*

I now appeal to the reader to decide if there can, by any possibility, be a grain of philosophical truth in this supposed result of "interference," so explicitly taught by Professors Tyndall, Helmholtz, and all writers on sound. If not, then, as a necessary consequence, the wave-theory breaks down, having no foundation on which to rest. I must say here that with one moment's thought Professor Tyndall himself could not help but admit that the two organ-pipes named would be heard precisely the same in line when 14 feet apart as when separated 28 feet, or rather a trifle louder, since the farthest pipe would be nearer the listener when separated from him by only half a wave-length. To say that this eminent savant would deny that the pipes could be heard in line when 14 feet apart, or that he would still insist on his law of "interference" and "silence" after his attention was directly called to the question, is to assert what I do not and can not believe till such time as the Professor shall flatly compel me to do so.

It will not do to say that though we may hear the sounds of these pipes thus stationed half a wave-length apart, it is not their *fundamental* tones we hear, but their principal *over-tones*, and that this law of "interference" only supposes the neutralization of the *primary sounds* of the two instruments, whose waves are necessarily of the same length! This objection, though presented to me by a sound-expert of considerable reputation, is wholly foundationless, and can be set aside by a single fact, since any person, having two unison forks, and causing them to be sounded over two resonant jars of proper depth placed half a wave-length apart, can hear their tones exactly the same in line as at right angles, or when a whole wave-length apart; while according to the testimony of Professor Helmholtz, the very highest authority on the subject, *such sounds are destitute of accompanying over-tones!*

The truth is, there is no force whatever in the objection. Every one knows a *fundamental tone* from its *octave*, which is the *first* or *principal over-tone;* and by sounding any two unison pipes half a wave-length apart and listening in line, one can instantly tell by the evidence of his ears alone that the fundamental tone does not cease at all, neither is weakened, but is rather heard exactly the same in quality and quantity, according to distance, as when the pipes are a full wave-length apart, no difference whatever occurring in this respect; and a man who is not capable of comprehending the truth and force of this self-evident declaration never ought to let the sacred word "science" escape his lips.

But I do not need to depend upon argument, however conclusive, to show that no such thing as this so-called "*interference*" can take place between the sounds of two unison instruments stationed, as described by Professor Tyndall, half a wave-length apart. As has so often been done during this discussion, it is only necessary to quote another passage from the same authority in order to show the most startling and point-blank contradiction of the whole position here assumed in regard to "interference." I have frequently suggested that a radically false theory can not avoid self-contradiction, in the very nature of things, when it comes to the discussion of details, and here we have another illustration of it. I will now array Professor Tyndall against himself, producing a practical case of "interference" and "neutralization," and then let him or his friends settle it as best they can:—

"I have already had occasion to state to you that when *several sounds traverse the same air each particular sound passes through the air as if it alone were present.*"—*Lectures on Sound*, p. 281.

How, then, in the name of all that is called science, can two sounds "traverse the same air" in such a manner as to neutralize each other and produce "absolute silence" by the two systems of sound-waves *interfering*, when "*each particular sound passes through the air as if it alone were present*"?

We thus have the most overwhelming evidence from Professor Tyndall himself that all this reasoning about the possibility of the sound-waves of two unison forks neutralizing each other by so-called *interference* is a pure fabrication, without the plausibility of ordinary fiction; and hence that there is not the slightest foundation either for this law of "interference" or for the hypothetic sound-waves from which it is deduced, since it is evident if air-waves exist at all, two sounds would be just as apt to clash and neutralize each other as to be heard, making the last quotation clearly false.

The general conclusion, therefore, to which I am logically forced, is, that this eminent authority never tried this experiment at all, either publicly or privately, of sounding two unison instruments half a wave-length apart, and thus producing neutralization by this so-called law of "interference," but rather that he gives the illustration in his book, and explains this law on general principles, based on the blind assumption that it must be so, because the wave-theory must be true and necessarily requires it, when it would not have taken him half an hour to make a careful experimental test with two unison forks or other instruments, which would have instantly dissipated the delusion, and opened his eyes to the fact that this pretended law of "interference" in these so-called sound-waves is a pure and simple chimera, contradicted by reason as well as by the observation of all mankind.

Thus again, as so frequently witnessed during this discussion, one of the strongest arguments in favor of wave-motion in sound-propagation turns out, when unlocked by the combination key of truth and common sense, to be a magazine which explodes and annihilates the theory; for, as we all know that two unison instruments can positively be heard the same in any direction when sounded half a wave-length apart as when separated a whole wave-length or any other distance, as an illiterate rustic might easily ascertain, it follows that there is no such a thing as "interference" in sound-waves; and if no interference, then no waves to interfere, since water-waves, as every one knows, will interfere under just such conditions as this physicist lays down, and mutually destroy or neutralize each other, thus demonstrating the wave-theory to be a fallacy of science by the very argument advanced to maintain it!

"But do you deny the *interference* of sound under any circumstances, or such a thing as a *phase of opposition?*" I am asked by the intelligent scientific reader. I answer, emphatically, "Yes!" in any sense which could be analogous to the interference which takes place in wave-motion. A certain kind of interference or opposition resulting from a forced departure from unison in two instruments sounding in close proximity, as observed in so-called "beats," and caused by the same affinity which produces sympathetic vibration, is no doubt possible, and which I will try to elucidate before the close of this chapter. But prior to this, I undertake to meet and explain the principal class of facts relied on by physicists as favoring the common view of interference, as just exemplified in the argument about two unison forks, or as caused by supposed waves with condensations and rarefactions.

One of the strongest arguments favoring such a law is drawn from the action of the *double siren*, which, it is claimed, demonstrates beyond question that two systems of sound-waves from two unison *sirens*, operated together in such a manner as to cause alternation of sounds in what is supposed to be half wave-lengths, neutralize each other, and thus produce "absolute silence"; while it is also claimed that the same effect is observable in the action of light, under certain optical conditions in which two rays, by interfering, will neutralize each other and cause absolute darkness! It was this phenomenon, Professor Tyndall tells us, which first led to the Undulatory Theory of Light. His words are:—

"We have here a phenomenon, *which, above all others, characterizes wave-motion. It was this phenomenon*, as manifested in optics, *that led to the undulatory theory of light, the most cogent proof of that theory being based upon the fact that by adding light to light we may produce darkness, just as we can produce silence by adding sound to sound*."—*Lectures on Sound*, p. 259.

I propose to show, in a few moments, that this whole matter, as regards the *double siren*, is a clear misapprehension on the part of these writers, and that no such effects as they describe can possibly occur with this or with any other unison instruments,—that no such thing as "silence" is or can be caused by any possible combination of the two rotating disks of this instrument or the tones they produce, and consequently that both Professors Tyndall and Helmholtz have entirely mistaken the action of the *double siren*,—and that in attempting to explain it to favor this law of "interference," they have perpetrated one of the most glaring and laughable blunders recorded in the annals of science.

This language, I admit, must seem to a physicist almost if not quite preposterous, particularly with reference to Professor Helmholtz, who invented the very form of siren on which the experiments about to be examined were made. Is it possible, the reader may pertinently ask, that this eminent physicist and musician does not comprehend the action or acoustical effects of his own instrument? I answer that it is possible, and now undertake to clearly demonstrate it; while such a fact ought to be no more surprising, if proved, than the already demonstrated fact that the same acoustician utterly misapprehended the action of the violin *bow* in relation to that of the *string*, supposing the latter to normally move *ten times* swifter than the former, though he was, at the time he perpetrated this fiasco, a practical violinist, as reviewed at pages 95, 96, and onward. The question of fact, therefore, whether Professors Tyndall and Helmholtz have in a similar manner misapprehended the sonorous effects of their own favorite *double siren* shall stand or fall on its merits after their explanation has been fairly examined. As they both give substantially the same explanation of their experiments with this instrument, agreeing in every essential feature, I shall confine my strictures almost entirely to that of Professor Tyndall, whose language is more explicit, not having had to pass through the ordeal of a translation into English.

Before directly considering the explanation of this author, which is so confidently supposed to embody one of the most explicit proofs in favor of the law of *interference* in sound-waves, it will be quite necessary that I should describe briefly the simplest form of this modern acoustical instrument called the *siren*, and then show how two sirens are operated together, making what is known as the *double siren*, in order that this demonstrative evidence may be duly appreciated.

Imagine a circular disk, about a foot in diameter, secured to an upright spindle passing through its center. Then imagine 12 half-inch holes through this disk in a circle near its outer edge, and that these holes are equidistant apart. Now suppose that a half-inch pipe leading from a wind-chest is so adjusted that its open end presses against the lower side of this disk at the exact line of the circle of holes. This may be said to constitute a single *siren*.

The disk now stands still, and one of the 12 holes is exactly over the open end of the pipe. If air is forced through the pipe from the wind-chest, it will pass in a jet up through this aperture in the disk; but should the disk slowly revolve while the pipe remains fixed, it is evident that the orifice of the pipe will soon change from the aperture in the disk to one of the spaces between these perforations, thus cutting off its jet of air; and the disk continuing to revolve, a puff of air will occur as each perforation passes in line with the outlet of the pipe.

It is manifest that by a more rapid rotation of the disk the puffs of air will occur in more rapid succession, till, by increasing the speed of rotation, as is proved by the operation of the instrument, the puffs will succeed each other so rapidly as to blend into a continuous tone, resembling that of a whistle, the pitch of which becomes higher in the exact ratio as the speed of rotation is increased, which, of course, correspondingly increases the number of puffs per second.

It will now be understood that each one of these air-puffs is exactly the same thing as a separate *vibration*, or equivalent in effect to a single oscillation of a harp-string, tuning-fork, or any other sound-producing instrument. Each rotation of the disk, therefore, causes 12 puffs or vibrations; and should the motion of the disk be increased to 36⅔ rotations per second, it will exactly sound the letter A, which requires 440 vibrations to the second,—thus giving a beautiful demonstration of the universal law in acoustics—that the pitch of every fundamental sound, from whatever instrument, corresponds precisely to the number of vibrations in a second which generates the tone.

By means of a proper registering device, with a dial geared to the rotating spindle, the number of rotations of the disk in a minute to any particular pitch may be recorded, which, multiplied by the 12 holes in the disk and divided by 60 seconds in a minute, determines the number of vibrations per second, giving thereby the true pitch of the *siren* at that speed of rotation, and of any other instrument to which it may be compared.

A *double siren* consists in the attachment of another disk like the one described to the same spindle a foot or more above the lower one, but turned upside down so that their two sets of puffs project the air toward each other. The upper disk may be so secured to the common spindle that by turning a handle it may be adjusted so that its puffs or vibrations will occur simultaneously with those of the lower disk, or alternately, just as the operator may desire; or, which is the same thing, the pipe which conducts the air to the upper disk may be shifted backward or forward, causing the same effect. If the two disks or their pipes are adjusted to puff at the same time, or in synchronism with each other, the tones of the two disks are in exact unison, and will continue so no matter whether the disks revolve slowly or rapidly, or whether the pitch of the two tones is thus raised or lowered. But should the upper disk or its pipe be so shifted that its puffs will occur alternately with, or half way between, the puffs of the lower disk, then, instead of unison, we have that condition which Professor Tyndall calls a "phase of opposition," in which the two systems of waves are in "interference," with the crests or condensations from one disk coinciding with the furrows or rarefactions from the other, and in which condition the two sets of puffs neutralize each other, "and we have no sound."

I have now, if the reader has closely followed me in this explanation of the *double siren*, prepared him for Professor Tyndall's remarkable demonstration, in his own words, by which he proves that we "can produce *silence* by adding sound to sound," just as "by adding light to light we may produce darkness," and I especially request that the Professor's conclusive language shall be carefully perused. It is as follows (*Lectures on Sound*, page 291:—

"But in the case now before us, where the circle is perforated by 12 orifices, the rotation through 1-24th of its circumference causes the apertures of the upper wind-chest [I have simplified the description by supposing a single pipe leading from the wind-chest] to be closed at the precise moment when those of the lower siren are opened, and *vice versa*. It is plain, therefore, that the *intervals* between the puffs of the lower siren, which *correspond to the rarefactions of its sonorous waves*, are *here filled by the puffs or condensations* of the upper siren. In fact, the *condensations of the one coincide with the rarefactions of the other*, and the *absolute extinction of the sounds of both sirens is the consequence*."

The "absolute" self-contradiction and absurdity of this assertion immediately follows, in Professor Tyndall's own words:—

"I may seem to you to have *exceeded the truth* here; for when the handle is placed in the position which corresponds to *absolute extinction*, *you still have a distinct sound*. And when the handle is turned continuously, though alternate swellings and sinkings of the tone occur, *the sinkings by no means amount to absolute silence*. The reason is this: The sound of the siren is a highly composite one. By the suddenness and violence of its shocks, not only does it produce *waves corresponding to the number of its orifices*, but the *aerial disturbance breaks up into secondary waves* which associate themselves with the *primary waves* of the instrument, exactly as the harmonics of a string or an open organ-pipe mix with their fundamental tone. . . . Now, by turning the upper siren through 1-24th of its circumference, we *extinguish utterly the fundamental tone*. *But we do not extinguish its octave*."

Here, reader, we have the *demonstrative proof*, in a citation which is the most astounding confession of weakness and untenableness of position perhaps ever seen from the pen of a scientific writer. It only needs to be taken apart and looked at carefully to place this lecturer in a most unenviable light as a physicist.

He first assures us, in words of ringing positiveness, that we can "produce *silence* by adding sound to sound," and that this is "the most cogent proof" of the undulatory theory of light, as it can be shown in a similar manner that "by adding light to light we may produce *darkness*." He then brings forward the *double siren*, the only instrument adapted to this experiment of forced alternation, and gives us his "most cogent proof" that his former assertion was to be believed. After completing the experiment he tells his audience that "the *absolute extinction of the sounds of both sirens is the consequence*," and then innocently adds, "when the handle is placed in the position which corresponds to *absolute extinction you still have a distinct sound*," and "the sinkings *by no means amount to absolute silence*"; and finally, after a confused attempt at qualifying, to smooth off the "suddenness and violence of the shocks" of his contradictory statements, by "*secondary waves* which associate themselves with the *primary waves*," he sums up his "most cogent proof" by profoundly telling his class that "we *extinguish utterly the fundamental tone. But we do not extinguish its octave*"!

In the name of science and reason,—in the name of acoustics and common sense, —what should have been expected but this very result? By operating the two sirens together (making them practically but one instrument) in such a manner as to cause their puffs to occur alternately, *he actually doubled the number of puffs or vibrations*, which, as every tyro knows, *must necessarily raise the fundamental tone to its octave!*

With all the experiments in which Professor Tyndall had just been engaged, stopping off a string in the middle to raise its fundamental tone to the octave *by doubling the number of its vibrations*, yet he could not see that by placing the upper siren so that its 12 puffs should *alternate* with the 12 puffs of the lower siren he produced 24 puffs to each revolution, exactly the same as if he had used but one siren with 24 perforations instead of 12! This must necessarily be the case when the two disks are within sympathetic distance of each other, as I will soon clearly demonstrate. By thus doubling the number of vibrations he naturally and legitimately raised the two unison fundamental tones to their octave, and the most astonishing thing in the whole matter is that Professor Tyndall should have been so astonished at the result that he falls into utter confusion in attempting to explain it, and ends by the contradictory statement just quoted that "the *absolute extinction* of the sounds of both sirens is the consequence," "*but we do not extinguish its octave*"!

Instead of at once recognizing the octave tone as the proper result, and the very one to have been legitimately expected from doubling the number of puffs, he tries to account for it to his anxious auditors as one of the incidental and inexplicable "clang-tints" or "over-tones" of this "highly composite" instrument, resulting from its "*secondary waves* which associate themselves with the *primary waves*"!

Though I was not present at this remarkable lecture, I can imagine the Professor in a confused perspiration listening to the two disks of his *double siren* whistling out their melodious *octave* (the very thing, of course, they ought to do, only he did not know it,) and wondering what to say to his curiously anxious and equally confused audience of scientific students!

He finally stops the machine, and after collecting his demoralized thoughts for a moment, he says, in substance:—

"You have all observed, during this conclusive experiment, that the sounds of both sirens were *absolutely extinguished*, and that you did not hear the least tone. [Applause.] You may think, some of you, that I have not told the truth. Well, in fact, I haven't. You did hear the *octave*, but that, you must remember, is just the same as no sound at all, so far as my argument is concerned, and the reason why you *hear* it and you *don't* hear it [Hear! hear!] is because the double siren is a *highly composite* instrument, having a number of distinct tones and clang-tints that don't properly belong to its number of orifices, but are accidental, the same as a string or an open organ-pipe breaks up the air into *secondary waves* that associate themselves with the *primary waves* in such a manner that the *suddenness* and *violence* of the *shocks* make you think you hear it when you really don't. [Bravo!] But still I must confess that when the *handle* is turned to the point which would *indicate silence, you still hear a distinct sound*, and the sinkings and swellings *by no means amount to absolute silence*. [Students glance at each other anxiously!] But as that is only the *octave*, as before suggested, it, of course, as you all know, amounts to nothing, since the *fundamental tone is extinguished*. [Students reassured!] I trust, therefore, you all agree with me that this demonstration of adding sound to sound is complete, and that my former statement, on which the undulatory theory of light was so firmly established that the whole scientific world has adopted it, namely, that by adding sound to sound we may produce silence, has been fully sustained by the result." [Hear! hear!]

Seriously, was there ever a great lecturer so pitiably at sea in the midst of a simple scientific experiment, and that, too, with his own favorite and familiar apparatus? It need not surprise the reader in the least if the Professor, in his next course of public lectures on Sound, when stopping off a string in the middle to produce its octave, should suddenly become confused and tell his audience that "the *absolute extinction* of the sounds of both" halves of the string "is the consequence," though "*we do not extinguish its octave*"; and that the reason why "we hear no sound" is because "the sound of the" string is a "*highly composite*" one, and that "the suddenness and violence of the shocks" of the "secondary waves which associate themselves with the primary waves" produce a number of harmonics or over-tones not represented by the normal vibrational rate of the string proper, and thus cause the "absolute extinction" of the fundamental tone, though "we do not extinguish its octave"! This would be just as lucid as his explanation of the *double siren*.

Here, then, we have that "most cogent proof" of the undulatory theory of light, since the Professor can so clearly "*produce silence by adding sound to sound*"! If he is as successful in "adding light to light," there will be no question about his having produced "darkness," in one sense, at least.

Now, the only attempt which Professor Tyndall can possibly make to escape this crushing demolition of his explanation of the *double siren* is to assume that the 24 alternate and consecutive puffs, coming equally from the two disks a foot or so apart, do not produce the same effect of converting the fundamental tone into its octave as if all the puffs or vibrations emanated from one disk. I presume he will necessarily resort to this, if he speaks at all, to save himself and his theory, as there is clearly nothing else left for him to say, and hence I shall be obliged to cruelly snatch even this *straw* from the drowning physicist by quoting his own explicit admissions.

Before doing so I wish to reason one moment with the reader, to show the weakness of such a quibble. Let us suppose one of the *disks* of the *double siren* removed. I now ask, would not the fundamental tone caused by the 12 puffs of the other disk be exactly the same, if, instead of one circle of 12 holes, there were two circles of 6 holes each, supplied with wind through separate pipes? Manifestly the effect would be exactly the same so long as the puffs from the two circles alternated or occurred intermediately, making 12 consecutive puffs in regular succession at each revolution of the disk. Professor Tyndall would not think of questioning the truth of this proposition, unless he wished to excite the astonishment of every scientific thinker.

Then, this being admitted, would it not produce the same effect exactly, supposing the disk large enough, if the two circles of 6 holes each were a *foot* apart,—that is, supposing they continued to puff alternately as before? No one can doubt but that the same fundamental tone would result in either case, as with 12 orifices in one circle. Then, why should not the same thing exactly occur, if, instead of one disk with two circles of 6 holes each, there were two disks placed no greater distance apart than these circles, with 6 orifices in each, so adjusted that their puffs occurred in the same perfect alternation? Thus, link by link the chain of logic is being coiled around this fallacious explanation of the double siren. Although I do not expect the force of this reasoning to be acknowledged by Professor Tyndall, I propose to let him speak from his published lectures, and thus confess the absurdity of his whole argument:—

"The *puffs of a locomotive* at starting follow each other slowly at first, but they soon increase so rapidly as to be almost incapable of being counted. If this increase could continue until the *puffs numbered* 50 or 60 a second, *the approach of the engine would be heralded by an organ-peal of tremendous power.*"—*Lectures on Sound*, p. 50.

Query: Would it make any difference with this "organ-peal of tremendous power" coming from the distant engine, *should one half of the puffs come from the steam-cylinder on one side of the locomotive and the other half from the other—six feet apart—so they only alternated?* I do not think that even this lecturer would venture to assert, after his attention was called to the fact, that the "organ-peal" would depend in the slightest degree upon whether the puffs all came from one side of the locomotive or alternately from both sides, so there were 50 or 60 alternate puffs a second in regular succession! Hence, if his locomotive illustration contains a vestige of philosophical sense, it shows his complete misapprehension of the action of the double siren, and establishes the correctness of the explanation I have given, demonstrating that the true cause of the tone jumping from the fundamental to its octave was the shifting of one siren in such manner that its 12 puffs would occur intermediately between the 12 puffs of the other, thus making 24 puffs to each revolution of the spindle.

Professor Tyndall, the reader will recollect, attributes this octave not to the 24 *vibrations* caused by the 24 alternate puffs issuing from the 24 alternate orifices which he actually had right before his eyes and ears, but to some mysterious and indefinable breaking up of the primary air-waves which were produced by the 12 unison puffs "into *secondary waves* which associate themselves with the *primary waves of the instrument.*" Hence, he assures us that this particular *octave*, unlike all other octaves ever heard, was not produced by the required number of 24 *vibrations* at all, but by the disintegration of primary waves, though, as usual, it flatly contradicts his teaching in another place, where he says that *no octave*, from *whatever instrument*, can be produced without *doubling the number of vibrations which caused its fundamental tone!* Notice how explicitly his statements demonstrate his law of "interference," and cause their own "neutralization" by "mutual destruction":—

"Placing a movable bridge under the *middle of the string*, and pressing the string against the bridge, I divide it into *two equal parts*. Plucking either of those at its centre, a musical note is obtained, which many of you recognize as the *octave of the fundamental note*. Now, *in all cases*, and *with all instruments* [the *double siren*, of course, as well as others,] *the octave of a note is produced by doubling the number of its vibrations*."—*Lectures on Sound*, p. 90.

Hence, we have the clearest possible admission that the *octave* produced by the *double siren*, on which the Professor becomes so terribly confused, was actually caused, just as I have urged, by the required 24 vibrations or puffs to the revolution issuing from the two disks in alternation, and not by the breaking up of primary air-waves at all, since "in *all cases* and with *all instruments* the *octave of a note is produced by doubling the number of its vibrations*"! Was there ever a more direct self-contradiction perpetrated by a scientific writer?

To suppose Professor Tyndall, while attempting to explain the double siren to his audience, really unaware of this well-known law in acoustics, that doubling the number of puffs or vibrations would necessarily raise the fundamental tone to its octave (which he entirely ignores in his explanation), is a supposition at once astonishing and incomprehensible; because, as we have just seen, he clearly recognized the law when experimenting with strings, and could hardly have forgotten it. To suppose that he knowingly suppressed this true and only explanation of the octave (and thus imposed upon the intelligence of his audience) in support of his former assertion that "we can produce silence by adding sound to sound" would be cruel, if not wicked. The charitable view would therefore seem to be that though he knew the law and was aware of the facts, yet in the complexity resulting from the "secondary waves which associate themselves with the primary waves" with the "suddenness and violence of the shocks" from that "highly composite" instrument, he became temporarily demoralized, and lost sight of the legitimate solution. Hence, the confused explanation involving such direct contradictions of what he had taught on other occasions.

But here a difficulty confronts us. If this contradictory and absurd explanation was the result of a momentary confusion, how are we to account for the fact that he has since published to the world in a carefully prepared book every detail of that extraordinary, and, I may say, ridiculous analysis of the *double siren?*—and not only so, but has superintended the work through various editions and translations into a number of European languages, with not one alteration from the original fiasco? The charitable view I have taken here looks like breaking down.

And it is equally astonishing that of the hundreds of scientific students who listened to that lecture, and the tens of thousands who have since read his book, not one has had the temerity or the kindness to tell the Professor what was the matter with his favorite *siren*, who, if she had not absolutely "lured him to destruction," had triumphantly succeeded in turning his head with her fascinating music!

It really seems incredible that a scientist of such reputed ability could not have seen that this close proximity of the two disks of the *double siren* to each other—revolving only a few inches apart—was the true cause of producing this octave, especially in view of the fact that their 24 alternate and successive puffs were the exact number required for such a result. The shallow superficiality which was incapable of thus connecting the two series of puffs, making their effect the same as if issuing from a single disk, is as pitiable as it is surprising. The only serious and practical way of accounting for such want of scientific resource is the fact (as every one knows who has ever compared these *Lectures on Sound* with the work of Professor Helmholtz on the same subject) that the great German investigator *made the mistake first*, while Professor Tyndall, according to his uniform habit, took the whole matter for granted just because that eminent physicist had announced it as science.

Hence, because Professor Helmholtz had mistakenly employed this plain and legitimate octave of the *double siren*, generated by the requisite 24 vibrations or puffs, to illustrate his improved ideas of *over-tones*, there was, of course, nothing left for Professor Tyndall but to do likewise, and thus relegate this simple result of 24 vibrations or consecutive puffs to an indefinable atmospheric disturbance breaking up into secondary waves which associate themselves with the primary waves of the instrument, owing to the suddenness and violence of its shocks! He seemed to have become so infatuated with Professor Helmholtz, or this music of his *siren*, as to temporarily lose his memory, or he surely would have recollected what he had before so distinctly taught, as just quoted, that "*in all cases, and with all instruments, the octave of a note is produced by doubling the number of its vibrations*"! Had the "organ-peal of tremendous power," which the two cylinders of a locomotive might produce by sufficiently rapid alternate puffing retained a place in his memory he would never have been cajoled into such an unenviable plight by the superficial blunder of Professor Helmholtz, but would have been able to connect the alternate puffs of two disks only a foot apart into one system of 24 vibrations to a revolution as easily as he could the alternate puffs of two steam-cylinders six feet apart, which, as any one knows, could, if rapid enough, be legitimately combined to make an "organ-peal of tremendous power."

Look for a moment at the language of Professor Helmholtz, and note the family resemblance between it and that of Professor Tyndall:—

"The puffs of air in one box occur exactly in the middle between those of the other, and *the two prime tones mutually destroy each other*. . . . Hence, in the new position *the tone is weaker*, because it is deprived of several of its partials [over-tones]; but it does not entirely cease; *it rather jumps up an octave*."—*Sensations of Tone*, p. 246.

It seems that Professor Helmholtz even sets the example of *self-contradiction;* for how, in the name of reason, can "*the two prime tones mutually destroy each other*," when they *do not entirely cease*, *but rather jump up an octave?* If a man jumps up on the top of a fence, he is not *destroyed*, or *neutralized*, or *obliterated*, in any sense whatever. He has only exchanged a lower for a higher position! So the two fundamental unison tones of the two disks, caused by 12 puffs to the revolution, simply combine into one tone of 24 puffs to the revolution, which lifts it to a higher position in the musical scale, or, as Professor Helmholtz plainly puts it, the tone "jumps up an octave," without involving any such thing as mutual destruction or neutralization.

The reason why "the tone is *weaker*" in the "new position" seems to be a profound mystery to this eminent investigator, save on the supposition that it consists of the first or principal *over-tone* ("deprived of several of its partials"), which is always too weak to be distinctly heard by the unaided ear while the prime tone is being sounded. It of course never occurred to this standard authority on Sound that the reason why the octave was "weaker" was simply because it was constituted of a *single* series of 24 successive puffs or vibrations to a revolution, while the prime tone was composed of two series of 12 *double* or *unison* puffs which necessarily re-enforced each other, and by which means their intensity was increased *fourfold*, as already quoted from Professor Tyndall. The "weaker" character of this *octave* is thus beautifully accounted for according to my explanation of the *double siren*, and would have been easily comprehended by Professor Helmholtz but for his pet brood of *over-tones* which he was just nursing into life, and on which account he pressed into service the assistance of this "highly composite" *siren* as a kind of foster-mother. But he will learn when he reads this review, if not before, that she has at last discarded the whole family as too conspicuously illegitimate and outlandishly ungeneric for even foster-children.

I now propose to Professor Helmholtz, with all deference and respect, and through him to the scientific world, a simple practical test of this whole problem, by which to demonstrate either the truth or falsity of my explanation of the *double siren*, and which will also and equally demonstrate the truth or falsity of his own solution, since one or the other of our explanations must necessarily fall to the ground.

Suppose, instead of a *double siren*, such as already described, having *two* disks, we construct a *triple siren*, having *three* disks, each disk containing a circle of 12 orifices and supplied with wind by a separate pipe, all three being secured one above another to the same rotating spindle. It is evident, if the pipes leading to the three circles of orifices should be so adjusted that when the spindle rotates the three disks shall puff simultaneously that they will unitedly make only 12 puffs to the revolution of the spindle, and hence the fundamental tone will be an intense triple unison.

Let us now suppose that the spindle makes exactly 11 revolutions in a second, producing 132 puffs, or the precise number necessary to generate the fundamental note C, with the three disks puffing simultaneously, and consequently all sounding the same note in unison. According to the explanation of Professor Helmholtz, the disks are not only sounding this prime C, but they are also faintly sounding several over-tones of different degrees of pitch, though they are not distinctly heard, owing to the loudness of the prime note. The first or principal over-tone, in point of intensity, he tells us, is C^1, exactly an octave above the prime, and that it was this over-tone, "deprived of several of its partials," which was heard as the *octave* in the experiment with the *double siren* when the two prime unisons were mutually destroyed by "interference."

As we now have three disks of 12 holes each instead of two, we can easily make them all "interfere" by so adjusting their pipes as to make them puff in regular succession one after another, with the intervals equidistant apart, thus producing 36 consecutive puffs to each revolution of the spindle. Supposing the rotation to continue at the same uniform speed after the pipes are thus shifted, it is manifest that 36 successive puffs will occur in the time of 12 puffs before the change. What, then, must take place? I here announce to the physicists of Europe and America—and earnestly request these high authorities on Sound to show that I am mistaken—that not only will the prime C vanish from the sound, but the octave C^1 will also not be heard at all; and that instead of C^1, which was alone heard issuing from the *double siren* (being in that case the proper tone for the 24 puffs produced at each revolution), we will only hear from the *triple siren* the note G^1, or the fifth above the octave C^1, being the exact note corresponding to 36 puffs to the revolution under that uniform speed of rotation.

Will Professor Helmholtz accept the proposition here made, and join the writer in carrying out this test, by means of a *triple siren*, that the scientific public may know what to depend on? If he is as frank and candid a physicist and investigator of science as there is every reason to suppose him to be from his writings, he surely will not feel at liberty to refuse aiding in this conclusive solution of not only the action of the *double siren*, but also of the truth or falsity of this so-called law of "interference," as well as of the entire wave-theory of sound, since they all necessarily stand or fall together.

If this advanced scientist should deem the suggestion here made worthy of his attention, and if, on making this experiment, should find that the fundamental note C entirely vanishes as soon as the pipes are shifted so as to make 36 successive puffs to the revolution, he at once destroys this law of "interference" based on *half* wave-lengths and the coalescence of condensations with rarefactions, since in such a case as this it is only *third* wave-lengths, the pipes being shifted to speak at a third of an interval each from one fundamental puff to another.

Then, again, if he shall find that not only the prime C, but the octave C^1, is silenced, what, pray, has become of his first *over-tone*, which made all the music heard coming from the double siren after the two disks were placed in a phase of opposition? The three disks, when puffing simultaneously and producing the triple unison fundamental C, surely were sounding also their first partial or over-tone C^1, according to Professor Helmholtz! What, then, has become of these three unison first over-tones if they are not heard, which they will not be if my prediction is correct? They should be heard even louder than from the *double siren* after the shift takes place, having one additional re-enforcement.

Finally, if the only tone heard, after this so-called "interference," shall turn out to be G^1, a fifth above the octave C^1, and the very pitch of tone requiring the 36 vibrations to the revolution, as every physicist will admit, is there a scientific thinker on earth who would not at once decide that the explanation here given of the *double siren* as the cause of it *jumping up an octave* is the correct one, and that neither Professor Helmholtz nor Professor Tyndall understood the instrument they were exhibiting to the public or its acoustical effects?

As an evidence that this is a correct exposition of the problem, any acoustician will readily admit if the three disks should be perforated each with a circle of orifices in the following order—the lower one with 12, the middle one with 24, and the upper one with 36 holes, that when sounding together they would produce the chord C, C^1, G^1, if rotating with 11 revolutions to a second; whereas, if the lower and middle disks should be suddenly stopped off and silenced while thus revolving, the upper disk, with 36 orifices, would go on sounding G^1 precisely the same and producing the same intensity of tone as would the three disks if perforated with 12 holes each and if so adjusted as to puff in succession, as already described. It would be a singularly suggestive fact, to say the least, if this explanation, given by a writer who has never seen a *double siren*, should turn out to be the correct one, in opposition to the opinions of the greatest sound investigators of the age!

In conclusion, on this subject, I would say that I am entirely willing that the discussion shall end with the single experiment here suggested, and I feel sure that the intelligent reader will not hesitate to admit its extreme fairness as well as the conclusive character of such a crucial test as the one proposed of a *triple siren*.

As Professor Helmholtz owns a *double siren*—a luxury, by the way, entirely beyond the reach of this writer,—it would not seem to be a difficult or very expensive task for him to attach a third disk to the rotating spindle, half way between the other two, connected with a suitable air-pipe, for the purpose of carrying out the test here indicated; and it would seem to be the very least this learned authority should think of doing, in view of this formal arraignment and the arguments presented to support it, in order to satisfy the students of our colleges and universities that his claim to their consideration as a public instructor in matters of science is a just one; while he can rest assured that the same discerning and critical students will hold him rigidly to the charge of having wholly misunderstood the effects of his own instrument, till such time as this test is carried out, and the result shown to favor his exposition of these phenomena as published in the *Sensations of Tone*.

To expedite matters, the writer will gladly meet the entire expense of making this improvement in the double siren, if it would be any inducement to Professor Helmholtz, and can be communicated with at any time, or drawn on for the purpose through the American publishers of this book. I will only add that the foregoing suggestions are intended to apply equally to Professor Tyndall, who also, as I am informed, owns one of the Helmholtz improved *double sirens*.

From the last two arguments examined it becomes clearly manifest that writers on Sound have no fixed or definite idea of what they mean by this law of "interference," nor any settled views as to what constitutes a "phase of opposition," by which two systems of unison sound-waves may "neutralize" and thus "mutually destroy" each other, notwithstanding they make this assumed "law" a fundamental principle of the wave-theory, as they are unavoidably compelled to do on the ground of wave-motion. The truth of this charge against physicists, as to their indefinite and incongruous conceptions of their own theory, involving its most cardinal principles, needs no other confirmation than the self-evident contradictions embraced in these two illustrated arguments.

I refer, of course, to the manner in which "interference" is exemplified: first, by the two unison forks sounding "half a wave-length" apart,—by which means the *condensations* of one of the systems of air-waves are made to coalesce with the *rarefactions* of the other system, *regardless of the synchronism or alternation of their vibrations;* and then to the manner in which the same "interference" is explained by the action of the *double siren*, with its two disks puffing in *alternation* and mutually destroying each other's sound, *without the least reference to their distance apart!* The two explanations are not only clearly unlike, but are directly in conflict with each other, the two in turn mutually annihilating each other's pretended "interference," as a moment's consideration will show.

Let us, then, direct our attention to the two unison forks, placed half a wave-length apart, and first notice how they are said to produce their "phase of opposition" and the "mutual destruction" of each other's sound, with no regard to whether their vibrations occur *simultaneously* or *alternately*. Such a contingency as a possible *alterna-*

tion between the vibrations of these forks is not hinted at by the lecturer; and if it was thought of, it was cautiously concealed from the audience as too grave a difficulty to attack. Yet this circumstance,—the equal possibility of such *synchronous* or *alternate vibration*,—as will soon be seen, utterly breaks down and nullifies this law of "interference," because the two disks of the double siren are claimed to produce the same "phase of opposition" *alone* by *alternate vibration*, which the two forks do *alone* by vibrating *a definite distance apart!* Hence, the manifest self-disintegration of the two phases of this so-called "phase of opposition" which possesses such "marvelous flexibility," in the language of Professor Huxley, as to act on two opposite principles at the same time. A more suicidal law, I will venture to assert, never thrust its audacious claims into any scientific hypothesis. In one breath, "interference" and "mutual destruction" result alone from the two sounding instruments being placed half a wave-length apart, without reference to their equal chance of vibrating *alternately* or *synchronously*, while in the next breath,—only thirty pages further on,—the same "interference" assumes a new face as well as "phase of opposition," being caused alone by *alternation*, without reference to what distance the instruments may happen to vibrate from each other. Is it possible that a "law" can be relied upon as having any foundation in science which is first one thing and then another, as suits the caprice or emergency of a whimsical and self-contradictory theory? A pretended scientific "law" can surely have no substantial claims upon the consideration of any mind competent to reason philosophically, which is forced to change its very nature and mode of operation within thirty pages, under the manipulation of its ablest exponent, especially when such metamorphosis involves its own absolute self-neutralization, as I will now endeavor to illustrate.

First, as to the two unison forks sounding half a wave-length apart. Professor Tyndall explicitly tells us that a "condensation" from one of these forks, *owing solely to the fact of traveling "half a wave-length," reaches the other fork exactly in time to coalesce with its "rarefaction," without regard to whether the latter fork is at that instant sending off a rarefaction or a condensation!* Was there ever seen such a limping and imbecile hypothesis as this? Not a word, remember, as to whether the two forks swing in such relation to each other as to generate condensations simultaneously, or whether one fork shall generate a *condensation* at the same instant the other generates a *rarefaction!* The Professor ignores such a vital circumstance in this brilliantly defective explanation, for reasons perhaps known to himself; but it can not be ignored nor glossed over here. The simple and homogeneous idea of "half wave-lengths" seemed to be all this "highest living authority" was capable of grasping at one time. To have mixed up with such a profound problem the troublesome question of the possible alternate vibration of the two forks, which he must have known was just as liable to be the case as for them to vibrate simultaneously in the same direction, was evidently too much for him to undertake till such time as he should come to the *double siren*, thirty pages further on, when *alternation alone* should be the subject treated on, without any reference to that opposite kind of "interference" caused by "half wave-lengths"!

To prepare the reader for a just appreciation of this difficult task of mixing together two such incongruous *phases of opposition* and attempting to make them harmonize, let us first note the concise teaching of Professor Tyndall as to the manner in which a tuning-fork generates these so-called "condensations and rarefactions." This preliminary instruction is essential to a correct understanding of the problem of how two forks generate interference and consequent silence when separated "half a wave-length."

It is entirely evident that this lecturer had lost sight of his recent extraordinary teaching in regard to the prong of a tuning-fork "swiftly *advancing*," *compressing* the air "immediately *in front of it*," and *thereby* producing "a *condensation* of the air," and then "retreating" and "leaving a partial vacuum behind," by means of which "a *rarefaction* of the air" is produced, and that in this way the sound-waves, consisting each of a *condensation* and a *rarefaction*, are *carved* and *moulded* and *sent* off at a velocity of 1120 feet a second! (See page 264.) His uniform teaching, throughout his *Lectures on Sound*, is that a prong of a tuning-fork moving *outward* in either direction makes the "condensation" of the air, while the same prong moving *inward* makes the "rarefaction" of the air. Hence, the absolute indispensability of taking into consideration this circumstance, in connection with the half wave-length separation, in order to arrive at any rational or consistent hypothesis in regard to the law of "interference" between such "condensations and rarefactions," as exemplified by the action of two forks thus stationed. Had the manner, here described, of generating the "condensations and rarefactions" of sound-waves, which he had so carefully elaborated in a previous lecture, flashed across his mind while laboring to explain to his audience how two unison forks produce "interference" by simply being made to sound half a wave-length apart, he must, I am persuaded, have hopelessly broken down in the midst of his argument, unless he is a man of extraordinary nerve. The writer of this would have dematerialized under such a shock.

Let us now suppose that the two forks, half a wave-length apart, happen to oscillate *alternately*,—that is, suppose the prongs of one fork should swing outward, "rapidly advancing" and producing "a *condensation* of the air," at the same moment the prongs of the other fork "retreat" or swing inward, producing "a *rarefaction* of the air, which, as remarked a moment ago, they are just as liable to do as to both swing in the same direction, as Professor Tyndall well knows,—it is perfectly manifest that the *condensed* half of the wave from one fork would then reach the other fork (half a wave-length distant) just in time to coincide with *its* condensation instead of its *rarefaction*, thus producing complete *coincidence*, or the exact opposite of *interference*, which Professor Tyndall was trying to make out! Fully one half of the number of times, therefore, when tested, according to the law of chances, there would be absolute *coincidence*, and consequently a loud sound in the line of the two forks, while the other half of the time there would be *interference*, and no sound at all!

Clearly, then, "interference" by separation half a wave-length, depends entirely upon the accident of "coincidence" between the vibrations of the two forks. Discard this, and the law is a nullity. But as there is nothing in this pretended law of "interference" in the first place, as I contend, and no difference in the sound of two unison forks, whether they vibrate a half or a whole wave-length apart, as Professor Tyndall might have easily tested, it follows that we will never notice the least difference in the effects of two such sounding instruments, under the circumstances named, should we test them a million times.

From the foregoing analysis does it not clearly follow, if there is any foundation for Professor Tyndall's solution of the *double siren* and its peculiar mode of producing interference by *alternate vibration*, that such action completely *neutralizes* the *neutralization* caused by the supposed half wave-lengths, thus converting *interference* into *coincidence*, and *vice versa*, just as the two forks might chance to oscillate either in synchronism or in alternation?

It is also plain to see that the same self-neutralization follows us into the supposed "interference" of the *double siren*, claimed to be caused alone by the alternate vibrations or puffs of its two disks, but which has already been shown to be no interference at all, being simply the proper and legitimate mode of *jumping up an octave* by doubling the number of its vibrations, as any sensible *siren* would do if attempting to raise its pitch an octave higher. We have only now to bring to bear upon this phase of opposition the principle of interference involved in the idea of "half wave-lengths" to also neutralize its neutralization! Let us just see how scientifically and logically one destroys the other, the same as in grammar two negatives neutralize each other and become equivalent to an affirmative.

Suppose the two disks of the double siren (instead of being placed on the same spindle one above the other) stationed side by side 51 inches apart, or just half the wave-length of the note C, which requires 132 vibrations to the second, making a whole wave-length 102 inches, and suppose the two disks so geared together and their supply-pipes so adjusted as to puff alternately. Of course, according to the explanation given by Professors Tyndall and Helmholtz the two disks are thus in a "phase of opposition," at whatever rate of speed they may revolve, and hence their puffs must neutralize each other alone by the operation of one disk producing a "condensation" at the exact time the other produces a "rarefaction," or, in Professor Tyndall's own words, "In fact, the *condensations* of the one coincide with the *rarefactions* of the other, and *the absolute extinction of the sounds of both sirens is the consequence*"*;* and that, too, remember, without the least intimation as to what distance the two sirens are to be separated, or whether there is to be any distance at all between them. In fact, no amount of distance whatever separating the two disks could by any possibility enter into the calculation of this mode of "interference," since these physicists teach that the same phase of opposition continues as the speed of rotation increases and the pitch rises, which would cause a constantly varying "half wave-length" to be necessary between them, if any such thing were taken into account. Hence, with the two disks of the *double siren*, the "interference," the "phase of opposition," and the "absolute extinction," are effected exclusively by puffing *alternately*, whatever distance they may be apart. But here steps in the other phase of this suicidal "law" of interference growing out of the "half wave-length" theory, and vetoes all this nonsense about "alternation"; for the moment the two disks are made to revolve fast enough to generate the note C, it is manifest that the *condensation* from one disk, by traveling half a wave-length, or 51 inches, will reach the other disk in time to exactly catch or coalesce with its *condensation* just starting, thus producing "*coincidence*" instead of "*interference*," and thus again *neutralizing* Professor Tyndall's *neutralization* or "absolute extinction" by producing the precise *opposite* of his supposed "phase of *opposition*"! Was ever the self-stultification of a theory more beautifully elucidated?

We thus see that this pivotal "law" of the wave-theory, as explained by Professor Tyndall, and as made to bear upon two separate phases of his hypothesis, completely neutralizes itself; and, instead of favoring the idea that sound has anything to do with wave-motion, the assumption, by this strained effort to frame some kind of interference between imaginary systems of air-waves, simply results in the overthrow of the current sound-theory, by proving that air-waves, with *condensations* and *rarefactions* as the basis of sound-propagation, have no existence in Nature, unless it be a purely fanciful existence in the imaginations of physicists. This demonstrative and all-pervading "law" which a moment ago seemed so efficiently active in favor of wave-motion,—producing "interference" between systems of undulations which had no practical existence,—and which was so flexibly accommodating as to create a "phase of opposition" in almost any direction, to order, has, under cross-examination, literally broken down the whole wave-theory by hopelessly arraying the most conclusive arguments of these physicists against themselves.

If Professor Tyndall could succeed half as well in establishing "mutual destruction" between two systems of sound-waves under the action of this so-called law of interference as he has done in producing a "phase of opposition" and "neutralization" between his most powerful arguments, he would have succeeded at least a score of times in rendering the wave-hypothesis invincible, as the foregoing pages amply illustrate.

But I have another and still more startling proof of the self-neutralizing effects of this supposed law of "interference" between the *condensations* of one system of waves and the *rarefactions* of another. To demonstrate the complete self-destruction of the principle involved, we need go no further than to Professor Tyndall's own reiterated description of the manner in which these "condensations" and "rarefactions" are generated and sent off from a tuning-fork or harp-string, and then look at the legitimate result of such generation and propagation.

Each fork or string, according to these explanations, produces *two distinct systems of sound-waves*, one system being sent off from one side of the fork or string, and another system being at the same time sent off from the other side, *the same motion producing a rarefaction on one side and a condensation on the other*, and each system being constituted of the same kind of "condensations and rarefactions." Observe the conciseness and unmistakable character of his language:—

> "Imagine *one* of the prongs of the vibrating fork swiftly advancing; it *compresses the air immediately in front* of it, and when it *retreats* it leaves a *partial vacuum behind*."—*Lectures on Sound*, p. 62.

Of course, on the opposite side of the fork the same thing takes place precisely, the other prong sending off the same kind of condensations and rarefactions in the opposite direction. This no one will pretend to dispute. Now, would it not be a surprise to Professor Tyndall, and to physicists generally, if it could be shown from this language that these two systems of waves, sent off from the two opposite sides of the fork, must necessarily *interfere* and *neutralize* each other, thus producing "absolute silence" according to the wave-theory? I will here undertake to demonstrate, to the satisfaction of any one who will attentively read this short argument, that two such systems of waves must necessarily *interfere*, and hence should result in "absolute silence," if there is the least foundation for the theory of wave-motion in the propagation of sound. But first notice the equally explicit teaching of this same high authority in regard to the vibration of a single *harp-string*, which is much less difficult to comprehend than the somewhat complex operation of the two prongs of a tuning-fork:—

"Figure clearly to your minds a *harp-string vibrating to and fro;* it *advances*, and causes the particles of air *in front of it to crowd together*, thus causing a *condensation of the air.* It *retreats*, and the air-particles *behind it separate more widely*, thus producing a *rarefaction* of the air."—*Heat as a Mode of Motion*, p. 372.

It is plain to see from this language that both the "condensation" and the "rarefaction" here named are generated and propagated by this "to and fro" motion *on one side the string only*, and we have then only to "figure" another system of the same kind of condensations and rarefactions, generated in the same way, and sent off from *the other side of the string*, and then ask, *What takes place directly above the string?* Ah, that's the rub! Professor Tyndall never thought to explain this missing link in his favorite theory of condensations and rarefactions. He could think far enough ahead to elucidate, as he did with the row of glass balls, the *carving* and *moulding* of waves on one side of the string, and their propagation in a straight line, but, as was the case with the glass balls, he makes no provision for the air-particles slipping up or down, to the right hand or to the left. There being no motion of the harp-string "to and fro" in a *vertical* direction, of course there can be no *crowding of the air-particles together* as it advances, nor *separating more widely* as it retreats; hence, no condensations nor rarefactions *up* and *down*, and consequently no sound-waves, since sound can only exist and be heard as such condensed and rarefied waves.

Hence, it follows that no sound should be heard above the string at all, according to the wave-theory, since there is no advancing nor retreating in that direction to *carve* and *mould* the required condensations and rarefactions. Is it not, therefore, the legitimate teaching of Professor Tyndall, and also of the wave-theory, of which he is the most popular exponent, that the sound of a harp-string should not and can not be heard above the string at all, since there is no motion to and fro in that direction? This must be clearly the doctrine of the theory, since without motion there can be no "*condensation of the air*," and without condensation there can be no *air-wave*, and without air-waves there can be no *sound!*

But here *Nature* steps in, as usual, and contradicts the unavoidable logic of the wave-theory, since it is well known to every observer that sound is heard in a vertical direction, or directly above the string, just as intensely and at as great a distance as horizontally, or in the direction the string oscillates,—*which simply annihilates the assumption that sound is in any way connected with such supposed condensations and rarefactions*, or that they are necessary for its existence.

Now, the only possible answer to this difficulty is that the *lateral* or *horizontal* air-waves, as they are sent off from the string, *re-act* and *reflect upward*, thus conveying their condensations and rarefactions to the regions of air above the string as well as in a horizontal direction, the *row of glass balls* to the contrary notwithstanding. But here is exactly where "interference" and self-neutralization come in, as promised a moment ago, and which I will now make good.

It must be remembered that the *condensation* on one side of the string is generated and sent off by the very identical motion which generates and sends off the *rarefaction* on the other side of the string, and at exactly the same instant of time; so that, according to the theory of "interference" by half wave-lengths, recently reviewed, the *rarefaction* on one side of the string would re-act and reflect upward a given distance, just in time to coalesce with the *condensation* from the other side, since they occur synchronously, and both travel with the same velocity, of course; and hence the two systems of waves from the two sides of the string must necessarily produce complete interference and cause "absolute silence" in a vertical direction, if there is the shadow of truth in the wave-theory! Thus, every way it can be presented, it is proved to be a monstrous self-contradiction, unworthy of a moment's serious attention by any well-informed physicist, except so far as to expose its superficiality and overthrow its claims as a scientific hypothesis.

I now invite the attention of the reader quite briefly to the question of musical "beats," with which most musicians are familiar, especially those accustomed to tuning instruments. They occur when two sounding bodies are slightly out of unison, and consist of a sensible increase of intensity, followed by a decrease almost to inaudibility. These swellings and sinkings of the tone occur once for each complete vibration difference in a given time between any two sounding bodies. In other words, if the vibrational numbers of two tuning-forks, for example, are respectively 256 and 257 per second, there would be but one beat per second. If the difference between them should be two complete vibrations in a second, there would be two beats. If there was a difference of only one vibration in five seconds, there would be, of course, but one beat or one sinking and swelling of the tone in five seconds, and so on. This is all the explanation needed, even by the unscientific reader, as to what beats are, and the cause of their number of recurrences in a given time.

The important question, however, which now concerns us, and which has puzzled physicists in all ages, from the time of Pythagoras to the present, is the true physical solution of these phenomena. We know, for example, that beats are produced by the difference in the vibrational rate of the two sounding bodies, and consequently by such sounding bodies being brought alternately into a relation of coincidence and opposition. But in what manner, or on what acoustical principle, does this change from coincidence to opposition between such instruments generate this successive increase and diminution in the intensity of the tone? On general principles, and as a matter of course, it is attributed by advocates of the current sound-theory to the *interference* of the two systems of *air-waves* sent off by the two beating instruments, though in what manner it is possible for two systems of hypothetic air-waves to interfere so as to produce this alternate sinking and swelling can not be made intelligible to an unscientific mind, or even to the advocates of the wave-theory, since, as just shown, the supposed coalescence of condensations and rarefactions amounts to nothing at all, by absolute trial, producing not the slightest effect when two instruments are placed half a wave-length apart; while the whole assumption is shown to be completely self-neutralizing whenever this supposed interference is combined with the same interference caused by the alternate puffing of the double siren.

That two systems of *air-waves*, if they exist at all as the means of sound-propagation, can not interfere so as to affect the intensity of sound in the slightest degree, Professor Tyndall tacitly admits in the passage recently quoted. "When *several sounds*," he says, "traverse the *same air, each particular sound passes through the air as if it alone were present*"; whereas, if the current theory of "interference," or the mutual destruction of sound by opposing air-waves, was true, as taught by physicists, any two sounds of the same pitch and intensity traveling together would be just as apt to travel in interference and cause absolute silence as to coincide and be heard, the chances of course being equal. This has been repeatedly urged, and in various ways, as a self-evident fact which must alone be sufficient to break down all this reasoning about the interference of suppposititious air-waves, and of itself proves that beats are in no way connected with any such "phase of opposition." If such interference between air-waves were possible, then, clearly, the language quoted above, from Professor Tyndall, could not be true. Whatever, therefore, may be the true cause of beats, it is clear that the interference of air-waves has nothing to do with them.

Besides, it must be clearly manifest to the reader who has attentively perused the preceding arguments, that air-waves as the means of sound-propagation have no existence in fact, but are purely chimerical, being based on a complete misapprehension of the physical laws. This has been shown in so many ways that it is unnecessary to specify any particular class of arguments bearing against the hypothesis, since almost any one of the preceding two hundred or more pages, if opened to at random, will show facts and reasons against such a supposition which must convince an unbiassed scientist that air-waves are utterly inadequate to account for the phenomena of sound.

If, then, the scope and logical bearing of the arguments advanced in this monograph unanswerably disprove air-waves as the cause of sonorous propagation, it is folly to claim that these alternate sinkings and swellings of sound, as observed in beats, come from the interference of that which has no existence in fact.

It is the explicit teaching of every writer on sound, as all well-informed students of acoustics are aware, that the loudness or intensity of tone results alone from the swinging to and fro of the air-particles, with greater or less amplitude, as they strike the tympanic membrane, hitting it with a harder or a lighter blow; and hence that the sinking or swelling of a sound, as in beats, *takes place at the ear of the listener by this motion of the air-particles.* According to this universal teaching, it is not produced directly in the action or condition of the two instruments themselves, except so far as they act to mould and send off the waves of air, but is caused by the interference or coincidence of the air-waves themselves, after they leave the sound-producing bodies. I will refer to a few brief passages to refresh the memory of the reader. Professor Helmholtz says:—

"A periodically oscillating sonorous body produces a *similar periodical motion, first in the mass of air, and then in the drum of our ear.*"—*Sensations of Tone*, p. 16.

Professor Mayer teaches the same thing:—

"It is evident that the ultimate effect of the passage of sonorous waves through the atmosphere *will be to cause the molecules of the air to swing to and fro with the motions of pendulums.* It is also apparent that all the characteristics of the periodic motion at the source of the sound *will be impressea on the surrounding air, and transmitted through it to a distance.*"—*Am. Ency., Art. on "Sound."*

Professor Tyndall is even more explicit, if anything, on this subject. He says:—

"The *greater volume* of sound heard everywhere throughout the room *can only be due to the greater amount of motion communicated to the air of the room.*"

"We have already learned that what is *loudness* in our sensations, is, outside of us, *nothing more than width of swing or amplitude of the vibrating air-particles.*" ["*Nothing more*" excludes the sounding body itself as having any direct connection with this increase or diminution of sound, except as the mechanical means of sending off the air-waves!]

"The pitch of a note depends *solely* on the number of *aerial waves which strike the ear* in a second. The *loudness* or *intensity* of a note *depends on the distance* within which *the separate atoms of the air vibrate.* This *distance* is called the *amplitude of the vibration.*"—*Lectures on Sound*, pp. 48, 73.—*Heat as a Mode of Motion*, pp. 225, 372.

In another place Professor Tyndall distinctly says that if we hear one sound *louder than another* it is because the ear is "hit harder" in the one case than in the other by the vibrating air-particles (*Lectures on Sound*, p. 11). It is therefore easy to see that the sinking and swelling of the sounds of two beating instruments result "alone," according to the wave-theory, from the alternate *coincidence* or *interference* of the air-waves themselves sent off from such sounding bodies. I deny that this is any explanation at all of musical beats, as it has been clearly shown a few pages back that no such interference between two supposed systems of air-waves can take place, since not the slightest weakening of two unison tones occurs when two vibrating bodies are sounded half a wave-length apart,—the position which, above all others, admittedly meets this condition, and causes the *condensations* of the one system to exactly coalesce with the *rarefactions* of the other, if any such systems exist. Hence, this so-called "amplitude" or "width of swing" of the air-particles in the propagation of sound, in which they are said to oscillate "to and fro with the motions of pendulums," and to "shake the drum of the distant ear," is demonstrated to have no actual existence in Nature.

To show that "beats" are directly *caused, according* to the current theory of sound, by this alternate *interference* and *coincidence* of supposed *condensations* and *rarefactions* sent off in the form of waves, as the two beating forks *oppose* or *re-enforce* each other, I will quote Professor Tyndall's very clear and concise explanation of these phenomena, according to the received view of sonorous propagation. I will, however, first let him explain to the reader how these "condensations" and "rarefactions" from two unison forks, by *interfering*, may "abolish the sounds of both":—

"I draw my bow across a tuning-fork, which for distinction's sake I will call A, and cause it to send a series of *sonorous waves through the air.* I now place a second fork, B, behind the first, and throw it also into vibration. From B waves issue which pass through the air already traversed by the waves from A. It is easy to see that *the forks may so vibrate* that the *condensations* of the one shall *coincide with the condensations of the other*, and the *rarefactions* of the one *with the rarefactions of the other.* If this be the case, the two forks will *assist each other.* The *condensations* will, in fact, *become more condensed*, the *rarefactions more rarefied*, and as it is upon the *difference of density* between the *condensations* and *rarefactions* that *loudness* depends, the two vibrating forks thus supporting each other will produce a sound of *greater intensity than that of either of them vibrating alone.* It is, however, also easy to see that the two forks may be so related to each other that one of them shall require a *condensation* at the place where the other requires a *rarefaction;* that one fork, for example, shall urge the *air-particles forward* ["*swiftly advancing*"] while the other *urges them backward* [retreating and "leaving a *partial vacuum*"]. If the opposing forces be equal, particles so solicited will move neither backwards nor forwards, and the *aerial rest which corresponds to silence is the result.* Thus it is possible by adding the sound of one fork to that of another *to abolish the sounds of both.*"—*Lectures on Sound*, p. 258.

Here, then, as before stated, the cause of silence is the "interference" of the two systems of air-waves sent off from the two unison forks traveling in such relation to each other that the *condensations* of one system coalesce with the *rarefactions* of the other, thus tending to "abolish the sounds of both." Silence, in this case, has nothing to do with the *alternate* vibration of the two forks, as was the case with the so-called interference produced by the *double siren!* We will now let this lecturer tell us how to manipulate the two unison forks so as to make one vibrate a trifle slower than the other, and thus generate the "beats" of which we are seeking an explanation. The reader will carefully note that the alternate *swellings* and *weakenings* of the tones of the beating forks, as here described, are explicitly attributed, all the way through, to the alternate *coincidence* and *interference* of the *condensations* and *rarefactions* of the *air-waves:*—

"Each of the two forks now before you executes exactly 256 vibrations in a second, and when they are sounded together you have the perfect flow of unison. I now load one of them with a bit of wax, thus causing it to vibrate a little more slowly than its neighbor. Supposing, for the sake of simplicity, that the wax reduces the number of vibrations to 255 in a second, what must occur when the two forks are sounded together? *If they start at the same moment, condensation coinciding with condensation* and *rarefaction with rarefaction*, it is quite manifest that this state of things can not continue. The two forks soon begin to exert *opposite actions on the surrounding air.* At the 128th vibration *their phases are in complete opposition, one of them having gained half a vibration on the other.* Here the one fork *generates a condensation* where the other *generates a rarefaction;* and the consequence is that the two forks, at this particular point, *completely neutralize each other, and we have no sound.* From this point onward, however, the forks support each other more and more, until, at the end of a second, when the one has completed its 255th and the other its 256th vibration, the state of things is what it was at the commencement. *Condensation then coincides with condensation* and *rarefaction with rarefaction*, the full effects of both sounds being produced upon the ear. . . . It is quite manifest, that under these circumstances we can not have the *continuous flow of perfect unison.* We have, on the contrary, *alternate re-enforcements* and *diminutions* of the sound. We obtain, in fact, the effect known to musicians by the name of '*beats*,' *which, as here explained, are a result of interference.*"—*Lectures on Sound*, p. 262.

Thus, consistently, all the way through the wave-theory, these authorities explain *beats* as the alternate *interference* and *coincidence* of the *condensations* and *rarefactions* of air-waves after they have been generated and sent off from the fork, and that when the weakening of the tone occurs it takes place alone because the tympanic membrane is not "hit" so hard by the oscillating air as when the tone is louder.

To make sure that the reader shall comprehend this pivotal fact of my argument, namely, that "beats" occur alone by the alternate motion and quiescence of the air-particles, I will make one other reference to Professor Tyndall's explanation. He says:—

"In the case of *beats* the *amplitude of the oscillating air reaches a maximum and a minimum periodically. . . . Its particles alternately vibrate and come to rest.*"—*Lectures on Sound*, pp. 266, 268.

Now, in opposition to this explanation of beats, I maintain that the operation which alternately augments and diminishes the intensity of tone, as the oscillations of the two forks cross each other's path in changing from synchronous to alternate vibration, has nothing to do with air-waves or any motion of the air-particles whatever, but *takes place in the instruments themselves, or in their potential and practical sympathetic attraction for each other*, without regard to the *coincidence* or *interference* of such useless nonentities as these so-called atmospheric *condensations* and *rarefactions.* I claim that the simple laws of acoustics, as applied by the consistent principles of the corpuscular hypothesis, which have thrown light on so many mysterious phenomena and elucidated so many difficult questions during the preceding discussion, will be found amply sufficient, when properly investigated and analyzed, to clear up this occult problem of "beats" on the general law of *sympathetic vibration.*

At pages 79, 80, &c., I endeavored to show that the *sympathetic vibration* of a fork or string, when its unison was sounded near it, could not, by any rational possibility, be accounted for on the supposition of the synchronous dashing of air-waves against it, as the wave-theory necessarily assumes, and gave what I consider good and sufficient reasons for rejecting such an hypothesis, even if no arguments had since been advanced showing that such atmospheric sound-waves have no real existence in Nature. I assumed, as the only consistent view, that there exists potentially, in all bodies capable of producing a musical sound, an affinity or sympathetic attraction for all other bodies capable of such sonorous effects, the same as there exists potentially in a piece of steel a magnetic sympathy for all other bodies of steel, and that it only requires that mysterious electric condition which we designate as *magnetic*, to cause such unison steel bodies to either attract or repel each other, according to the manner in which their magnetic currents of substantial but intangible corpuscles synchronize or cross each other's path. In an analogous manner, a sounding body only needs to be tensioned to that rigidity which develops a *unison* relation to other bodies of like sonorous rigidity, to raise its potential affinity into a practical sympathetic attraction, and by which means its potential or dormant sonorous pulses are taken hold of by the corresponding pulses of its unison neighbor, which gradually cause it to awaken into a similar sonorous action. And in a manner very analogous to this principle of magnetic repulsion, when the relation of polarity is reversed so that the substantial magnetic currents oppose each other, two forks or other sounding bodies, if made to vibrate in such a manner as to be thrown *periodically into* and *out of* unison, by oscillating first together and then in opposite directions, may alternately attract and repel, sympathize and conflict, re-enforce and oppose, each other, by the coalescence or interference of their substantial corpuscles acting upon each other's sonorous potentiality, quite similar to such magnetic action.

I will not pretend here to enter into the minutia of this hypothesis, which, it seems to me, will, when properly elaborated, fully explain the phenomena of beats on the principles of the alternate re-enforcement of, or interference with, this sonorous affinity or sympathetic attraction between two musical instruments, and which will, as I believe, prove to physicists much more satisfactory than the superficial and illy considered supposition of air-waves. I simply throw out the general suggestion of this law of sympathetic attraction as the rational basis of a solution, to show the reader that this problem of beats, as one of the most relied-on arguments of physicists in favor of some kind of interference between air-waves, is no exception to the general rule that such assumed "phase of opposition" is as useless as it is impracticable, and as foundationless as the air-waves on which it depends.

I will only present a single argument to show, as I believe, conclusively, that the action and force which produce beats are to be traced to the instruments themselves, and their influence upon each other, and need not be carried a single inch away to accommodate this superficial hypothesis of interfering air-waves. Suppose, for example, two forks mounted upon their resonant cases and tuned sufficiently out of unison to produce, say, one beat to the second. If sounded in close proximity to each other, or, as my hypothesis teaches, in a position of strong sympathetic attraction, a listener stationed a hundred feet away from them will distinctly hear their *beats*,—will, in fact, hear them as far away as the sounds of the forks are audible. But let the two forks, while sounding, be gently separated only a few feet toward the right and left of the listener, and though he will continue to hear their united sounds in full force, yet the beats will entirely cease, showing that they result from the sympathetic influence of the two forks upon each other, owing to their affinity, and not to the alternate *interference* and *coincidence* of the two systems of supposed air-waves a hundred feet away, or at the ear of the distant observer, as the wave-theory teaches.

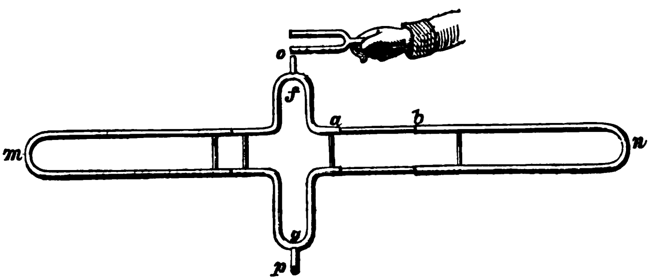
It is perfectly plain that the two systems of air-waves from two beating forks, if such waves exist at all as the cause of sound, must travel to the distant observer exactly in the same relation to each other (as to coincidence or interference) when the forks are slightly separated to the right and left, remaining equidistant from him, as when their resonant cases are in such close juxtaposition as to actually touch each other. Yet, in the former case, when not in close sympathetic proximity, the sounds are as perfectly smooth and mellow as if they flowed from two forks in absolute unison; while in the latter case, when in close sympathetic union, the beats can be distinctly heard, as before remarked, to the extreme limit of audibility. Need there be any stronger argument required to show that the alternate coincidence and interference of hypothetic air-waves are in no way whatever connected with the cause of sonorous beats? And need there be another argument adduced to show that the true cause of these phenomena lies, as here postulated, in the influence of the two instruments upon each other through this law of sympathetic attraction, as required by the corpuscular hypothesis?

Following the lead of this assumed "interference," we would naturally expect it to finally culminate in something like direct evidence of its existence, if it really has any such foundation in fact or science. It would be very strange, indeed, if an important "law" in physics, lying at the very basis of a scientific theory, and involving such an unmistakable condition of things as the occurrence of "*absolute silence*" between two loudly sounding instruments by the *interference* of their air-waves, should not be susceptible of some sort of demonstrative proof which appealed directly to the auditory sense, instead of depending on mere theoretical inferences, which might vanish into thin air the moment we attempt to practically test them, as was the case with the assumed interference between two unison forks sounding half a wave-length apart, recently examined.

In our search after something practical and tangible of this sort, we have at last found it, in the shape of an acoustical apparatus manufactured by M. Konig, of Paris. This ingeniously constructed instrument is intended to squarely meet the difficulty by dividing a stream of sound into two unequal branches, one being half a wave-length longer than the other, and then re-uniting them in a common outlet, where they must naturally be expected to interfere by the *condensations* of one of the systems of waves coalescing with the *rarefactions* of the other, thus producing the long sought for "absolute silence" so essential to this "law," and so indispensable to the wave-theory of sound as a scientific hypothesis.

It is needless to say that such a conclusive proof of the current hypothesis of wave-motion as this would be, if founded on fact, would naturally receive considerable prominence in Professor Tyndall's book, as it certainly does. Before making any further remarks in regard to the apparatus or its acoustical effects, I will take the liberty of transferring bodily to these pages the engraving and explanation, as given by this author, and earnestly request the reader to carefully examine the same:—

"Sir John Herschel first proposed to divide a stream of sound into two branches, of different lengths, causing the branches afterwards to re-unite, and to *interfere* with each other. This idea has been recently followed out *with success* by M. Quincke; and it has been still further *improved upon by M. Konig*. The principle of these experiments will be at once evident from Fig. 141. The tube *o f* divides into two branches at *f*, the one branch being carried round *n*, and the other round



m. The two branches are caused to re-unite at *g*, and to end in a common canal, *g p*. The portion, *b n*, of the tube which slides over *a b* can be drawn out as shown in the figure, and thus the sound-waves can be caused to pass over different distances in the two branches. Placing a vibrating tuning-fork at *o*, and the ear at *p*, when the two branches are of the same length, the waves through both reach the ear together, *and the sound of the fork is heard*. Drawing *a b* out, *a point is at length attained where the sound of the fork is extinguished*. This occurs when the distance *a b* is one fourth of a wave-length; or, in other words, *when the whole right-hand branch is half a wave-length longer than the left-hand one*. Drawing *b n* still further out, the sound is again heard; and when twice the distance *a b* amounts to a whole wave-length, it reaches a maximum. Thus, according as the difference of both branches amounts to half a wave-length or to a whole wave-length, we have *interference* or *coincidence* of the two series of sonorous waves. In practice, the tube *o f* ought to be prolonged till the direct sound of the fork is unheard, the attention of the ear being then wholly concentrated on the sounds *that reach it* through the tube."—*Lect. Sound*, p. 261.

After it had fallen to my lot to discover so many inaccuracies, and, it may be justly said, inexcusable mistakes, in the scientific observations and experiments of this physicist, it was quite natural that I should be inclined to discount in advance this entire statement in regard to the Konig instrument. It was plainly evident to my mind, if the apparatus and its acoustical effects were correctly described they would strongly favor the wave-theory, and would present an almost conclusive evidence in favor of this law of interference between sound-waves, as claimed by advocates of the hypothesis. I therefore, on general principles, could not believe that the representation, as quoted, was truthful to any degree which would tend to favor the theory of wave-motion, for the reason that I had already found so many considerations bearing directly against it which were absolutely unanswerable; and because, as all science and reason plainly teach, *a true theory can not contradict itself*. I was therefore compelled to assume, in advance, on the same general principles of logic, that, should any sonorous change be observed, on drawing out one branch of this instrument half a supposed wave-length longer than the other, it would be susceptible of a satisfactory explication on some other hypothesis than that of wave-motion.

In view of these considerations I resolved to test the matter carefully, and now have the satisfaction of announcing that I have done so with the following conclusive results.

To make entirely sure of my data, I first obtained from a friend the use of a complete Konig instrument (the one represented in the engraving), and tested it with forks of different vibrational numbers, carefully drawing out, while each fork was sounding, the sliding branch (*b n*) of the device in order to detect the exact point of *silence*, as recorded by this high authority on sound, if any such point existed. But I here declare to the reader and to the scientific world that no such thing as *silence* occurs, nor even a respectable approach toward it. By the most careful act of attention, while moving the sliding branch of the instrument backward and forward, a point was discovered which produced a slight though sensible weakening of the tone, but it required care to detect it. This, however, is very far from justifying the extravagant language of Professor Tyndall, just quoted, namely, "Drawing *a b* out, *a point is at length attained where the sound of the fork is extinguished.*" This is not true, in any pardonable sense of the word "extinguished," since the sound of the fork is not diminished in intensity more than about one quarter, as any sound-expert would readily admit. So much, then, for the reliability of Professor Tyndall's scientific statements when recording simple matters of fact, on which no one need be or can be mistaken, if he has ever tried the experiment.

But here comes the important question, What causes this sensible weakening of the tone as the sliding branch of the instrument is drawn out to a certain point, if there is no truth in the wave-theory or in this law of interference between sound-waves? This is an inquiry which must naturally suggest itself to the mind of the reader, and, in arriving at a correct answer, it will be found, as I now propose to show, that physicists have wholly misapprehended this instrument and its acoustical effects, as was so clearly proved to be the case with the "phase of opposition" in the *double siren*, and that this change of tone has nothing to do with air-waves or their supposed interference. The attention of sound-investigators is especially invited to the solution about to be given, which will no doubt be new to acoustical science.

By means of one specific test (with which all others agreed), I found that a fork with 256 vibrations in a second, and a consequent wave-length of 52 inches, sounded at *o* (see engraving), required the sliding branch *b n* to be drawn out *not* sufficiently to make *half a wave-length difference* in the two branches (26 inches), but exactly 24 inches, in order to produce the maximum change or diminution of intensity. This would make the whole wave-length from such a fork but 48 inches, instead of 52 as it should be; that is, if this enfeebling effect was actually due to the interference of two systems of air-waves, as Professor Tyndall teaches. Besides, if this weakening of tone was the effect of a genuine *interference* between the *condensations* of one stream of sound and the *rarefactions* of another, there should be "absolute silence," as all physicists teach, since the two wave-systems are *exactly equal.* But as there is a reduction only of a scarcely noticeable fraction of the normal intensity of the tone of the fork, which reduction takes place at a point differing materially from the half wave-length hypothesis, it follows that the phenomenon, whatever it may be, must be explained on some other principle than that of so-called "interference" between two systems of atmospheric sound-waves. Is not this mechanically, acoustically, and mathematically, incontrovertible?

I will now undertake to give a solution of this phenomenon, without resorting to any such incongruous laws and facts as those involved in the explanation based on the assumption of wave-motion, and will endeavor to explain how this solution was arrived at.

I became satisfied, on finding that the difference between the two branches, at the point of greatest diminution of sound, was 24 instead of 26 inches, that the effect must be the result of *resonance*, and therefore must be due to either the re-enforcement or opposition of the two vibrating air-columns of the two tubes, the same as just explained in regard to the cause of beats. To strengthen this surmise, I found that the same fork (256 vibrations) held over the mouth of a tube open at both ends, required 24 inches as its maximum resonant depth, or a depth corresponding exactly to the difference between the two branches *m* and *n*, thus proving, incidentally, that a tube open at both ends is somewhat more than double the resonant depth of a similar tube having one end closed; and thus again showing the habitual inaccuracy of Professor Tyndall's observations, who teaches that the length of one tube is exactly double that of the other.

The fact thus discovered, that the maximum resonant depth of a single open tube agreed precisely with the difference in the length of these *two* open tubes forming the Konig instrument, my next effort was to invent some means of verifying my conclusion, and thus demonstrating that it was not the "interference" of two *streams* of sound-waves, but an effect of resonance which caused this perceptible weakening of sound. Fortunately the invention came to me, and accordingly I constructed the Konig instrument, with the important difference of elastic branches (*m* and *n*) formed of rubber tubing, which could be attached and detached of any required length, and stopped off at any desired portion of either branch.* I ascertained by the first test that precisely the same effect was produced with the elastic tubes as with those of the Konig instrument, and that the greatest diminution was reached, as before, when the difference in length was 24 inches instead of 26 inches, or a half wave-length.

Retaining this proportion of length between the two branches, my next experiment was to take advantage of the elastic tube by pinching it together, between my thumb and finger, at various places, while the fork was sounding at *o*, and observing the result with one branch open and the other closed; and, to my surprise and gratification, I found that my suspicions were correct, and that I could obtain exactly the same result of weakening the tone by stopping off the short branch between 11 and 12 inches from *f*, thus having but one stream of sound instead of two! I thus demonstrated the fact that at this particular point there was not the slightest difference in the intensity or quality of the tone when the sound passed through both branches and "interfered," as supposed, and when it passed through the long branch alone, and resounded back in opposition from the short tube closed at one end. I made this conclusive test by pinching and relieving the tube in rapid succession, thus suddenly changing from two streams to one; but not the least difference could be observed, as just remarked, in the quantity or quality of the sound, the same effect being produced by the opposing resonance of one open and one closed tube as was produced by the opposing resonance of two open tubes with a resonant difference of 24 inches in length.

To complete the demonstration that there was nothing in this supposition of "interference" between the two streams of sound or their supposed air-waves, I adjusted the two branch tubes to exactly equal lengths, which, of course, produced the full resonant effect of both tubes.

* The improved Konig instrument, with elastic branches, here referred to, can be seen at the office of HALL & Co., publishers of this book.

Then, by simply pinching one of the tubes, as before, at about 12 inches from *f*, I obtained the same weakening of tone precisely as was observed when the branches differed by 24 inches, or when the two streams were in supposed "interference"! I thus clearly proved that dividing the stream of sound into two branches of unequal lengths, and again causing their air-waves to unite and "interfere," was a pure misapprehension of physicists, and amounted to nothing at all in favor of wave-motion, since a single continuous stream gave exactly the same result when opposed by a closed tube of a different resonant depth.

This weakening of tone caused by the two branches of the tube differing half a supposed wave-length, as well as the effects of the test last given, will no doubt be found, when fully understood, to be only the result of coalescence or opposition between two resonant columns of air of different vibrational numbers, which reënforce or oppose each other by the law of sympathetic attraction, in a somewhat analogous manner to the attraction and repulsion of two magnets, as recently intimated, and as illustrated in musical beats. At all events, the hypothesis of two streams of sound "interfering" by the *condensations* of the one system of waves coalescing with the *rarefactions* of the other, is completely exploded by these experiments with the elastic tube improvement on Konig's instrument, which show that any resonant effect produced by dividing the sound into two streams can be equally obtained by a single stream, as just described, in connection with a closed resonant tube of certain depth.

Aside from this solution of the problem, it remains an unassailable fact that no such thing as *silence* or any approximate approach toward it takes place when one branch is half a wave-length longer than the other. I emphasize this fact, in opposition to the authority I am quoting. What, then, must be thought of the statement of Professor Tyndall, in which he distinctly says that when drawn out to the difference of half a wave-length, "*the sound of the fork is extinguished*"? He either deliberately and knowingly misrepresented the facts of the case, or else he taught and published to the world on mere inference or hearsay, as science, that of which he had no personal knowledge, because it seemed to favor the hypothesis of wave-motion! It is the safest and altogether the most charitable view to assume that he never tested an apparatus of the kind, and possibly never *saw* one; for it is altogether probable, if he had ever seen one of these Konig instruments, his curiosity would have induced him to test it, and thus correctly inform himself as to its sonorous effects. How he dared venture to make such baseless explanations of an apparatus he had never tested, and which was so easily obtainable, baffles human ingenuity to conceive.

In addition to this altogether probable supposition, I now venture the assertion, without knowing the facts, that the Royal Institution of London, under whose auspices these lectures on Sound were delivered, does not own one of these Konig instruments, or at least did not at the time of their occurrence, since it is more than probable that if such a device had been among the scientific apparatus of that institution some one of the members would at some time or other have had the curiosity to test it, and would thus have been enabled to enlighten Professor Tyndall, who evidently stood in such pressing need of it.

It is a singularly incongruous fact that this eminent author takes especial pains to commend scientific investigators who *shirk no pains or labor* in arriving at the exact truth, wherever it may lead them, or whether it favors a pre-adopted theory or not, and who never take anything for granted in science on mere *theory* or *inference* when an *experiment* is possible to verify or contravene it! His eulogistic commendations of physicists who thus labor are so praiseworthy that I must quote one or two sentences:—

"Those who are unacquainted with the details of scientific investigation have no idea of the amount of labor expended in the determination of those numbers on which important calculations or inferences depend. . . . There is a *morality* brought to bear on such matters, which, in point of severity, is probably without a parallel in any other domain of intellectual action. *The desire for anything but the truth must be absolutely annihilated; and, to attain perfect accuracy, no labor must be shirked, no difficulty ignored.*"—*Lectures on Sound*, p. 26.

Why did Professor Tyndall, after employing such beautiful language as this in commendation of faithful workers in science, *shirk the labor* of testing the Konig instrument, which he might have readily obtained, before publishing to the world a scientific description of its effects having not a shadow of foundation in truth, thus practicing a breach of that "morality" which he commends in others, and deceiving the young scientific students of the land, who look to him as a guide? Why did he *shirk the labor* and *ignore the difficulty* of testing two unison forks or other sounding bodies placed half a wave-length apart, by which he could have convinced himself that not the slightest difference occurs in their sounds from such inferential or theoretic "interference," when it would not have taken him half an hour to make the experiment, and completely overthrow the wave-theory? Instead of acting on this principle of fidelity to scientific truth, which he had so highly eulogized in others,—that "the desire for anything but the *truth* must be absolutely *annihilated;* and, to attain *perfect accuracy, no labor must be shirked, no difficulty ignored,*"—he found it altogether more available and convenient to deal in scintillating theoretics, for which he is so noted, about the "interference" of hypothetic *air-waves*, which have no real existence in Nature, and thus "shirked" the trifling labor of sounding two forks at different distances apart, while his assistant observed in line their acoustical effects! The truth is, he could not help knowing that his theory of "interference" would have appeared to much better advantage had he been able to demonstrate it before his audience by producing "absolute silence" between two unison instruments sounding half a wave-length apart. But for some reason, which I leave the reader to find out, he did not attempt any such a fatal experiment. In connection with this manifest *shirking of labor*, I beg the reader to note his pen-painting of a "true physical philosopher":

"The *true physical philosopher* never rests content with an *inference* when an *experiment to verify or contravene it is possible.*"—*Lectures on Sound*, p. 36.

Yet he was "content" to assume, on mere theoretic "inference," the most important and pivotal facts of the current sound-theory, when an "experiment," costing but a few minutes of his time, would have not only *contravened* such assumptions, but, in doing so, would have annihilated the whole theory, since the assumed facts named constituted the very key to the main arch of the superstructure.

He not only rested "content" to shirk the labor of an "experiment" to test the truth of many of his most fundamental hypotheses, but in some cases he even spent more time in *fixing* an experiment to favor his theory than it would have taken to make an honest experiment, and thus "contravene it"! As a proof, look at his effort to put the "smoke of brown paper" into "one end" of his tin tube, so that no "puff" should be ejected from the other end on clapping the books, when it would have cost less care, at least, to fill the whole tube by elevating its small end, and thus to have shattered his experiment! (See page 270, and onward.) The absolute annihilation of a "*desire for anything but the truth*" did not seem to apply to this case, and clearly demonstrates that the experimenter was not a "*true* physical philosopher," according to his own definition, or he would not have "shirked the labor" of filling the whole tube, and thus have rested "content with an *inference*" when an "experiment" was at hand to "contravene" the hypothesis!

These animadversions may seem unkindly severe; but, as a "true physical philosopher," I dare not "ignore" nor "shirk" the responsibility of exposing such unreliability in the discussion of scientific phenomena. I am forced, in truth, to assert that no careful and competent observer can fail to be astonished, on reading Professor Tyndall's various scientific works, at the continual recurrence of the most glaring inaccuracies everywhere visible. I open accidentally, as an illustration, to page 49 of his *Lectures on Sound*, and see this prominent "law" announced:—

"To produce a *musical sound* we must have a body which *vibrates with the unerring regularity of the pendulum.*"

Yet a more erroneous proposition was never penned in a scientific work, since it can be shown that a highly "musical sound" may be produced, in which no two of its vibrations are of the same periodicity! To make sure that the above statement was not a slip of the pen, he repeats it on the next page in even stronger and more explicit language. He seems to do this to impress it upon the reader, that, under no circumstances, can there be an exception to the rule:—

"The *only condition* necessary to the production of a *musical sound* is that the pulses should succeed each other in the *same interval of time*," or, as before expressed, "*with the unerring regularity of the pendulum.*"

The fallacy of this carefully reiterated law can be shown in a single sentence. The motion of a pendulum, as every one knows, is perfectly *isochronous;* that is, it oscillates with exactly the same periodic intervals, when once started, from its longest to its shortest swings, or until it settles entirely to rest; whereas, the most "musical" of all the sounds produced in an orchestra, as every musician is aware, are the *sliding* tones of the *violin* or *violoncello*, in which no two vibrations are of the same periodicity, and hence are the very opposite of isochronous or pendulous, as to intervals of time!

But why spend time in pointing out and criticising the philosophical views of a writer who tacitly admits himself not to be a "true physical philosopher," by not conforming to the requisites he has himself prescribed?

While thousands of scientific students are to-day ready to accept almost any proposition relating to the advanced theories of the time, if they only know it to have the indorsement of Professor Tyndall, I declare to the reader, upon my conscientious conviction, that, from the evidence of the quotations in these pages alone, it would be a safe general rule to reject, as probably fallacious, any scientific theory of which he might have become a prominent champion. Of course there are exceptions to most general rules, and it would be strange if even a uniform tendency to inaccuracy should not occasionally diverge into the truth.

I might continue these direct and damaging quotations *ad libitum*, had I space, as *there is not an instance in this whole course of lectures on Sound, where the truth of the wave-theory is directly involved in the explanation*, which could not be equally turned against the lecturer and made to militate against the current hypothesis of sound. But the fatal instances already given are a sufficient illustration of the blinding influence of a false theory in leading the greatest intellects into error, even on the simplest questions of fact.

And here I feel compelled to say that it has been extremely unpleasant, and even embarrassing, though a moral and scientific necessity in my case, as explained in the preface, to be forced to take issue with such unqualified antagonism with so eminent a scientist, especially on simple questions of veracity and fact,—such as those concerning two unison forks sounding half a wave-length apart, and the acoustical effects of the Konig instrument,—questions in regard to which the possibility of being in error is so utterly unnecessary that it is difficult to conceive of anything short of an unpardonable want of information, which could have superinduced such reckless assumptions and such erroneous statements. Yet this very explanation of the engraving just reproduced from his book, and this action of two unison forks in abolishing each other's sound when placed half a wave-length apart, are but the legitimate fruits of the wave-theory, being no more foundationless than any other part of the hypothesis, and no less conspicuously and distinctly inculcated by every other writer on Sound, in proportion to his ability, than by this physicist.

However, it must be regarded as a matter of congratulation to the scientific world, as well as to the general public, that this great authority has narrowed down the whole question as to the truth or falsity of the wave-theory of sound to a few simple and representative questions of fact, which need not depend for a single day on any man's veracity or scientific standing. For example, this single representative question of "interference" between air-waves, in which the whole wave-hypothesis is intrinsically involved, namely, whether two unison forks, or other instruments, if sounded half a wave-length apart, with the ear stationed in line, can be heard the same as in any other position, must absolutely settle the whole undulatory problem, now and forever. *If they can be heard the same in that as in any other position, which the whole world knows to be a fact, then the wave-theory falls to pieces, and with it falls Professor Tyndall as a scientist!*

It may seem unduly severe thus to select out for a target the scientific reputation of one physicist, who is but equally involved with others who have written on the subject of sound. But, in determining the basis of my arguments against the undulatory theory, I was compelled to choose for my principal antagonist a strictly representative English authority to quote from, that my review, after being completed, might not fall flat from not having touched the bottom facts of the hypothesis, or from having failed to grapple with the "highest living authority." I therefore selected Professor Tyndall (in connection with Professor Helmholtz, the representative German, and Professor Mayer, the highest American authority), recognized by the civilized world as the most eminently popular exponent of these various scientific theories,—particularly that of sound,—and whose lectures on the subject, from which my citations are made, have been translated into all the leading languages of Europe. If, therefore, he has fallen the fated victim upon the altar of progressive truth, to appease the wrath of the scientific gods, he may attribute the catastrophe to his having become a more conspicuous target than any of his coadjutors, by the greater triumphs of his genius in popularizing a theory having no foundation in Nature or true science, and no merit as a philosophical hypothesis save that imparted to it by the ingenuity of its advocates.

I will now briefly fulfill a promise intimated in the early part of this monograph, and that is to again call attention, at the close of the work, to the conspicuous and incongruous fact, that, while a fork or string in vibrating moves through the *air* at a velocity of only a few inches in a second, it actually "sends" off air-waves, as we are taught by physicists, at the enormous velocity of 1120 feet in the same time.

I have repeatedly urged, and given reasons for believing, as the reader doubtless recollects, that there can be no measurable spring-force to free air, while it contains no appreciable elasticity when unconfined by which a body moving through it can transmit a pulse to a distance, or stir the atmosphere even a short space in advance by causing one particle to push another, it another, and so on, as was illustrated by Professor Tyndall with his row of glass balls.

I also stated that this principle of *mobility*, one of the most prominent characteristics of our atmosphere, was of necessity ignored by physicists in their discussions of atmospheric wave-motion, since to recognize such a law, when assuming the transmission of an air-wave to a distance and at great velocity by a slowly moving fork or string, would be a fatal self-contradiction, as any kind of an impulse or atmospheric disturbance whatever must be counteracted and almost instantly neutralized by a persistent tendency to equilibrium.

Whatever displacement of the air-particles, therefore, may be effected by a vibrating string, such disturbed air can only travel, till it settles finall to rest, at a velocity equal to that of the displacing body. The aggregate distance traveled in a second, in one direction, by a vibrating prong or string, can not, as elsewhere shown, be more than seven or eight inches in a second.

It is true that some portion of the travel of a string in its oscillation to and fro is swifter than its mean velocity, owing to its tensile force added to its momentum; but how much swifter at its point of highest speed I have not been able to calculate to a certainty, nor have I been able to find any one who could aid me in determining this question to a nicety. If we even suppose its highest speed, at any one point of its travel, to be four times that of its mean velocity, which unquestionably exceeds the fact, and estimating but one half of the second occupied by its forward motion and the other half by its return motion, it would make its rate of velocity at the swiftest part of its travel but 64 inches in a second, or *not more than the one two hundredth part the velocity of sound.* This, manifestly, as the most ordinary mind must comprehend, is the utmost velocity an air-wave could attain, which receives its impetus from an object moving through the air at a speed no greater than that postulated above, as the highest point of velocity in a vibrating string.

Thus, while a string, estimating the swiftest portion of its travel, moves only at the rate of sixty-four inches in a second, it sends off its air-waves, as the current theory necessarily teaches, at a velocity of *thirteen thousand four hundred and forty inches in the same time;* or, in other words, it projects these aerial undulations through the air more than *two hundred times* swifter than the very motion which gives them their impetus! Was there ever anything taught as science more transcendently or transparently impossible than this? Yet, incredible as it may seem, this is the exact and unavoidable teaching of the wave-theory, which my friends have thought me almost if not quite insane for attempting to assail; while the most ordinary student must see that by no law of philosophy, and by no rules of mensuration known in heathen or Christian lands, could such a string "send" off corporeal waves of any kind of mobile substance *a distance of more than sixty-four inches in a second*, even if the friction and inertia of such substance were wholly abolished!

These facts more than bear out all I formerly said when presenting the fatal illustration of the locust. I then asserted that it must be evident to any thoughtful mind that the stridulation, so far from churning the entire atmosphere throughout four square miles into condensations and rarefactions, did not stir the air a *foot* around the insect, while what atmospheric disturbances did occur would not probably travel at a velocity greater than about *four feet* in a second. Had I placed it at *four inches* in a second I would have been much nearer the proper limit, that being the aggregate movement of the insect's legs in producing the tone. Yet it remains an unanswerable fact against the wave-hypothesis, that, while rasping its legs across the nervures of its wings, at this very slow rate of speed, the shrill tones which it produces are radiated over four square miles of atmosphere at a velocity at least *one thousand times greater* than that of the movement which generates the sound!

Should I, as a scientific teacher, publicly declare and impress it upon my hearers that a *bullet*, after leaving the muzzle of the gun, could travel with a velocity even two hundred times greater than that of the gases passing through the gun-barrel which gave it the impetus, as does Professor Tyndall virtually, and as he does *actually* in regard to air-waves, it could but reasonably be inferred either that I must assume my audience a convocation of idiots, incapable of distinguishing sound from light, whom I wished to test by stating a practical absurdity, or else that I had successfully demonstrated my own incompetency to handle any scientific question. If, however, after so teaching it, I should persist in maintaining it as true, and publish to the world as a settled fact of science that a bullet would travel thus *over two hundred times faster* than the gases giving it the impetus, which common sense would brand as a transparent absurdity, is there any language in which to frame a rebuke too severe for such a crime against science and human intelligence?

This mechanical law, which is applicable to all *physical* bodies,—*air-waves* the same as *bullets*,—does not apply to the incorporeal and almost infinitely attenuated emanations which my hypothesis assumes, and which constitute sound, light, heat, electricity, magnetism, &c.; for, though the vibrations of the fork generate these corpuscles of sound, they do not "send" them a hair's breadth from its prongs, any more than the effervescing of the acid or the decomposition of the zinc, which generates the electric currents, actually imparts to them their enormous velocity by the physical tremors of the battery!

I have carefully explained, in another portion of this review, that all such incorporeal emanations—as of sound, light, and heat,—acquire their velocity manifestly and alone from an unknown, and, as yet, inexplicable law of radiation, conduction, and diffusion, which is entirely independent of any vibratory or tremulous motion at their source, though to such motion their origin or generation is mostly if not entirely to be attributed.

No one knows, or can know, why electricity travels at such an inconceivable velocity through a wire, while no one would even for a moment suspect that it was caused by any corresponding physical movement at its source, any more than the vegetable tremors among the petals of the rose or honeysuckle were the means of imparting the velocity to their imponderable granules of fragrance, causing them to diffuse themselves through the surrounding atmosphere at considerable speed. It is equally irrational to suppose that the slight movement of a tuning-fork or string, but a distance of a few *inches* in a second, can project, as we have seen, sound-pulses two hundred times swifter than such vibratory motion through a substance absolutely devoid of appreciable spring-power when free to circulate, as is the case with air, which is the clearest possible demonstration that such pulses can not be constituted of air-waves, since the physical laws of mechanics hold with invariable uniformity as to the movements of all tangible and corporeal substances, such as air-waves or water-waves, where an equal and adequate mechanical motion and force are necessary for displacement and velocity.

A steamboat-wheel, for example, can not by any possibility "send" the waves of water from it, even if there were no inertia or friction to be overcome, at a velocity exceeding that of its revolving paddles. What would be thought of a scientist, of world-wide fame as a public lecturer, who should teach and then publish in a book that such a steamboat-wheel would actually "send" the waves of water away from its revolving paddles two hundred times swifter than their own movement? This is exactly what Professor Tyndall and all advocates of the current sound-theory teach in regard to vibrating strings, tuning-forks, &c., and the physical air-waves which they are supposed to "send" off! The bare fact that water-waves are admitted by Professor Helmholtz to be "essentially identical" with air-waves, ought to alone overthrow the wave-theory of sound, since water-waves can not travel faster than the displacing body which gives them their impetus.

To argue the point further than to thus clearly and distinctly state it in its proper bearing on this undulatory question, would be to assume the reader grossly ignorant of the simplest physical and mechanical effects. I will therefore close this argument by saying,—as Professor Tyndall will at once admit that the aggregate oscillatory movement of the fork referred to does not exceed sixty-four inches in a second, even counting its point of greatest speed, while the velocity of sound is 13,440 inches in the same time, or more than two hundred times faster than the motion of the fork,—that the demonstration becomes absolutely unassailable, namely, that these sound-pulses radiated from a vibrating instrument *are not constituted of air-waves at all*, and hence that the popular atmospheric wave-theory of sound has utterly and hopelessly broken down.

Lastly, in bringing to a close this somewhat extended review, I have the pleasure of presenting an argument which has been purposely reserved as a suitable culmination of this monograph. I trust it will not be considered unduly egotistical if I should declare as my deliberately formed conviction that the argument to which reference is here had is not only entirely original, but that, singly and alone, it is sufficient to break down the wave-theory of sound, even if the preceding portion of this treatise were blotted out; and I have no hesitation in further adding my belief that an unbiassed physicist can not help at once admitting the truth of this statement, after carefully reading the argument to which I refer.

This investigation of the nature of sound has already been extended to nearly double the number of pages originally contemplated, without exhausting the subject or presenting more than a tithe of the objections which might pertinently be urged against the current hypothesis. But a limit must be unavoidably reached at some point in the discussion, and I see no better way to fix upon it than with the single consideration here to be presented; though I have every reason to feel assured that sufficient has been already adduced to convince the candid and intelligent student of science that the wave-theory was originally founded on a clearly mistaken view of Nature's laws and forces. However that may be, I now invite the reader to the argument intimated, as follows:—

I have already had occasion, in discussing the cardinal laws and principles underlying the wave-theory of sound, to refer to the fact that there exists, according to the admissions of all writers on the subject, an absolute *analogy*, amounting to a clearly defined *parallel*, between so-called *sound-waves* and water-waves (see page 237, and onward). As the reader no doubt recollects, I quoted extended passages from Professor Helmholtz, the highest living authority on Sound, showing, in the most explicit language, that, according to the accepted view, *sound-waves* and *water-waves* are "of a *precisely similar* nature," are "*essentially identical*," and move "*exactly in the same way*." A single condensed extract will be here reproduced to facilitate the reader's examination:—

"Suppose a stone to be thrown into a piece of *calm water*. *Round* the spot struck there forms a little *ring of wave*, which, advancing equally in all directions, expands to a constantly increasing circle. *Corresponding to this ring of wave, sound also proceeds in the air* from the excited point, and advances in all directions as far as the limits of the mass of air extend. The process in the air is *essentially identical with that on the surface of water*. . . . The process which goes on in the atmospheric ocean about us is of a *precisely similar nature*. . . . The *waves of air* proceeding from a sounding body transport the tremor to the human ear *exactly in the same way as the water transports the tremor produced by the stone* to the floating chip."—*Sensations of Tone*, pp. 14, 15.

In view of the universal inculcation of physicists as to the nature of sound-propagation, of which this quotation from Professor Helmholtz but concisely expresses the substance, I need hardly say, that if, on a careful examination of the subject, it shall be found that *the essential elements of wave-motion are diametrically in conflict with the most prominently observed phenomena of sound*, does it need any further reasoning to show that the wave-theory itself is an unmistakable fallacy of science?

In the preceding argument, to which reference was just made, the reader will remember that the *amplitude* and *wave-length* of water-waves were proved to invariably sustain a relative proportion to each other, in feet and inches, of about 1 to 10, from the smallest ripples, having a wave-length of only an inch from crest to crest, to the largest ocean billows, having two and even three hundred feet of wave-length. This relative proportion was shown to belong to the very nature and necessity of wave-motion, involving principles and laws, which were pointed out, inseparable from such phenomena, whether in air, water, or any other fluid substance. Hence, when it was ascertained, by the clearest analysis of facts, that there was no *amplitude* at all, or oscillation of particles to and fro, in substances through which sound freely passes, such as the various metals,—not even enough to be observed with the aid of the most powerful microscope,—while the so-called *wave-lengths* of one of the low notes of the piano (E, with 40 vibrations to the second), according to the wave-theory, were absolutely 28 feet in air and 476 feet in iron, "from *condensation* to *condensation*," did it really require another argument to show to a critical scientific mind that no analogy whatever, or even an approach toward analogy, could exist between water-waves and so-called sound-waves? And was it not, therefore, a conclusive proof, that, instead of *undulatory motion* being the law governing sonorous propagation, sound travels in *direct lines* through all substances,—whether wood, water, air, or iron,—exactly as the corpuscular hypothesis requires, thus making it every way probable *that substantial sonorous pulses constitute the true and only solution of sound-phenomena?*

But now we come to that particular characteristic of water-waves to which I have been alluding,—one which is so inseparable from their very nature and existence, and so marked and easily determined, that it becomes conclusive on its face against the hypothesis of atmospheric sound-waves, by destroying the very idea of any analogy between the phenomena of sound and of true wave-motion; thus completing the destruction of the undulatory theory so effectually that even a child may, by means of this single argument, overwhelm the profoundest physicist. This peculiar characteristic of water-waves, and hence of all wave-motion, is the easily demonstrated fact, hitherto unobserved by any writer on sound, so far as I am aware, *that wave-velocity is always and exactly in proportion to wave-length, or distance from crest to crest!*

I assert, unhesitatingly, and am prepared to demonstrate it, that this is a characteristic of every conceivable system of waves within reach of our observation, and is so essentially interblended as a part and parcel of the nature and form of wave-motion, however generated, that water-waves can not exist at all outside of this concisely expressed law of Nature.

Thus, if the position I have here assumed be susceptible of unquestionable proof,—namely, that water-waves necessarily travel with a velocity proportioned exactly to their wave-length or distance from crest to crest, the large waves traveling many times swifter than the small ones,—it inevitably breaks down the wave-theory, as the unscientific reader can at once see, by shattering its very foundation of *analogy to wave-motion*, since it is a well-known fact, and universally admitted by physicists, *that there is no difference in sound-velocity between the highest notes, such as D of the piccolo flute, with a theoretic wave-length of less than three inches, and the low E, for example, of the double bass, with a theoretic wave-length, in air, of twenty-eight feet!*

In fact, the most casual observation of any one who has ever listened to a band of music playing at a distance of a quarter of a mile away, assures him full well that *the lowest and highest sounds produced must travel with the same velocity, since they reach the ear of the listener in perfect time*, the same as if he were stationed within a dozen feet of the players! Were this not the fact, or, in other words, were there any analogy between sound and true wave-motion, the music of a band would be utterly unintelligible if heard a single furlong away, as the *low* notes, with *long* wave-lengths, would outstrip the *high* ones, with *short* wave-lengths, destroying their rhythmical relation to each other, and consequently converting the most harmonious chords into a medley of discordant sounds. No one, with the least music in his soul, will doubt this, especially if he pretends to reason at all on questions of science.

Hence, it only needs to be shown, by positive observation and measurement, that large water-waves, having long wave-lengths, as with ocean billows, invariably travel with many times greater velocity than small waves, such as ripples caused by throwing a pebble into a still pond, in order to annihilate, by an infallible law of Nature, the very principle of wave-motion in sonorous propagation, because, according to the teaching of Professor Helmholtz and all writers on the subject, if sound-waves have any existence in fact, they should, as a matter of course, be "of a *precisely similar nature*" with water-waves, should be "*essentially identical*," and be propagated "*exactly in the same way*"! Clearly, then, if the *velocity* of water-waves is proportioned exactly according to their *wave-lengths*, while all sounds, as is universally known, travel with the same uniform velocity, without the least regard to their supposed wave-lengths, it must follow that instead of the two classes of phenomena being analogous, it makes them "*essentially*" *opposite*, "*precisely*" *dissimilar*, while they move "*exactly*" in a *different way!* It only, therefore, requires the literal *facts* in regard to wave-velocity to be settled in order to solve this whole problem of the nature of sound.

To determine the question involved in this final argument, and to leave no possible room for doubt as to these pivotal facts, I instituted a series of searching and careful tests, so that the matter could be presented to the scientific reader as the result of actual observation and measurement, and not as the result of a merely theoretic hypothesis, which, as we have so often found, may turn out to be fallacious and deceptive.

Accordingly, I began my investigations by testing the velocity of the smallest well-defined waves I could conveniently measure. To secure perfect stillness, I procured the use of a bath-room facing the south, so that the sun might shine through the glass upon the surface of the water. I then filled the tub (five feet long) with clear water, and arranged above it a pendulum of a suitable length to beat seconds; and, by so turning the faucet as to let the water drop about once in a minute, I had time to observe and measure one system of waves before another had commenced.

There was no trouble in accurately observing the movement of these tiny ripples passing off as a drop struck the surface of my miniature pond. I found, by repeated observations, that such wavelets were about one inch long from crest to crest, each drop producing about half a dozen well-defined undulations. Timing these waves by the motions of the pendulum, there was not the least difficulty in ascertaining that their velocity from one end of the bath-tub to the other was at the rate of *two feet in a second.* This was the inauguration of what turns out to be an important scientific discovery,—so important that it completely shatters an established scientific theory which had stood unshaken for centuries, and which no physicist has ever dreamed of calling in question.

My next observation was made on the surface of a still pond surrounded with high banks, so that no action of the wind might interfere with the accuracy of my measurements. A distance of 30 feet was carefully measured off, and while my assistant dropped stones into the water at given signals I timed the velocity of the waves sent off by noting the second-hand of my watch. The result was, after repeated experiments and much careful observation, that the wave-velocity, as well as wave-length, was proved to be in the exact ratio of the size of the stones dropped into the water,—those weighing about a pound driving off the waves the full distance of 30 feet in 10 seconds, or at a velocity of 3 feet a second. These waves I found to have a length of nearly a foot from crest to crest, and an amplitude of about *one inch*, measuring from the bottom of the trough to the top of the crest, as I judged, from the fact that such waves, 15 feet from my assistant, lifted the water around a stake half an inch above the normal level of the pond.

Incidentally, while experimenting in this way, I discovered another distinct error into which Professor Helmholtz had evidently been led by the misguiding tendency of a pre-adopted theory. In his anxiety to show that sound-waves and water-waves were "essentially identical" and "precisely similar," he was innocently (I will assume) led to misstate entirely the actual effect of dropping a stone "into a piece of calm water." In order to make this effect correspond to that of a single vibratory motion to and fro of a tuning-fork or harp-string upon the air, such stone, of course, must be made to produce but a *single wave*, with *a single crest and sinus*, since a single complete vibration of a sounding instrument, as all writers on sound tell us, generates but a single sound-wave, having one *condensation* and one *rarefaction*, both of which cease the moment the vibration ceases! Hence, it was absolutely necessary for Professor Helmholtz, in order to sustain the wave-theory, to leave the scientific impression on the minds of his readers that a single impulse thus produced on the surface of water by the impact of the falling stone would produce but a solitary wave! Accordingly, his language is very explicit, as just quoted: "Suppose a stone to be thrown into a piece of calm water. Around the spot struck there forms a little *ring* of wave, which, advancing equally in all directions, expands to a constantly increasing *circle*."

Now, it is evident that it would not have answered the purposes of the wave-theory, which this eminent physicist was trying to illustrate, to have spoken of *rings* of waves being thus produced, or of their expansion to constantly increasing *circles*, as this would not have been "precisely similar" to so-called sound-waves! But what is the fact? It is this, as any schoolboy knows who has ever thrown a stone into a pond, namely, that a stone, on striking the surface of water, *produces more than a dozen perfectly defined waves, which pass off in all directions, forming that many constantly increasing circles*,—thus, in a way wholly unexpected, showing an absolute dissimilarity and want of analogy between true wave-motion and these hypothetic sound-waves, even allowing physicists to fabricate them in their own way! It is entirely impossible to believe that Professor Helmholtz did not know that a stone thrown "into a piece of calm water" will actually produce a dozen or more well-defined waves. Why, then, did he speak of a single "*ring* of wave" and a single "*circle*"? I leave the reader to answer.

I next entered into a series of careful experiments, testing and measuring waves sent ashore from passing steamboats of different sizes, and traveling at various rates of speed. These waves were of correspondingly different amplitudes and wave-lengths, ranging from 8 to 20 feet from crest to crest, and from 10 to 24 inches from crest to sinus, thus keeping up a uniform proportion of about 1 to 10, in feet and inches, between amplitude and wave-length, as heretofore urged. To determine the matter carefully, my assistant took a position in a small boat 300 feet from shore, measured by a line which he kept taut; and, as the first wave from a passing steamboat would reach him, he would give me the signal, so I could note the time elapsing till it had reached the shore. By many such observations it was definitely established that exactly as the amplitude and wave-length increased did the velocity also increase, waves of a length of 12 feet from crest to crest traveling the distance of 300 feet in 40 seconds, or a trifle more than 7 feet in a second,—being more than double the velocity of the waves generated by dropping stones of a pound weight into still water, and more than three times the velocity of waves caused by drops of water falling into a bath-tub, as in my first experiment.

These facts were entirely conclusive to my mind that I had struck the lead which alone must overthrow and destroy the wave-theory of sound, since it was self-evidently impossible for that theory to be true, according to these tests and observations, unless it was a fact that tones of a low pitch, and having long wave-lengths, could be proved to travel with many times greater velocity than those of a high pitch and consequent short wave-lengths, which the observation of the whole world declares to be impossible, no difference whatever, as already shown, being observable between them.

It now only remained to test the velocity of ocean billows, or waves having a length from crest to crest corresponding to and representing tones of great depth of pitch, according to the wave-theory, such as the lower notes of the pianoforte and church organ. Accordingly, I took up my residence, for a period of time, at Rockaway Beach,—

"On old Long Island's sea-girt shore,"

so famous for its picturesque ocean billows and incessant surf. Wind and weather seemed to conspire to aid the cause of scientific investigation, as they gave me not only waves of all desirable dimensions, but the loveliest temperature conceivable in which to make my experimental observations and measurements.

By anchoring a couple of buoys, 200 feet apart, a short distance from the shore, and in line with the direction of the approaching waves, it was an easy matter to observe and follow the progress of any particular billow on which the attention was fixed, after it had lifted the farthest buoy, and thus note the exact number of seconds which would elapse before it would strike the other. It was a source of the deepest interest and congratulation, on the part of the writer, to watch from day to day, as the intensity of the wind varied, the absolute verification of this important discovery, as previously determined; for, as already observed, the velocity of these billows invariably increased with the exact ratio of increase in their size and wave-length!

For example, billows of about 4 feet amplitude and from 30 to 35 feet wave-length were 20 *seconds* in traveling the 200 feet, thus making their velocity 10 feet in a second; while rollers 8 or 10 feet high, and with a wave-length of 80 or 90 feet from crest to crest, actually increased their velocity to 15 or 16 feet in a second, or nearly *eight times the velocity of the small wavelets measured in my first experiment!* This was enough, though it was evident that, had I been able to witness and measure billows 20 to 30 feet high, and with a wave-length of over 200 feet, such as often occur in mid-ocean, their velocity would, by maintaining this ratio of increase, no doubt reach fully 30 feet in a second, or a speed of more than 20 miles an hour!

Now, with all these facts just as here presented, and which any student of science can easily verify by a little observation and at no expense, what has the advocate of wave-motion, as the scientific basis of sound-propagation, to say? There really seems to be but one single conclusion to which any logical mind can come, with these indisputable facts before it, and that is: As this fundamental principle of wave-motion demonstrates that the velocity of a system of waves is always in exact proportion to their wave-length, while the velocity of all sounds is the same whether their hypothetic wave-lengths are long or short, it follows, as a demonstrative scientific conclusion, against which no rebuttal can be made, that sound does not travel at all by wave-motion, and hence that air-waves, or the supposed undulatory motions of any other kinds of substance (through which sound is known to travel with great facility, such as iron, glass, wood, water, &c.), have nothing whatever to do with the generation or propagation of sound! Does it not, therefore, follow, as the inevitable result of these experimental observations, here for the first time placed on record so far as the writer knows, that the wave-theory of sound, in its fundamental principle and most vital element, is a scientific mistake based on a complete misunderstanding of the physical laws?

In addition to the foregoing decidedly conclusive results, I had the satisfaction of making and recording another observation while noting the progress and velocity of waves sent off from passing steamboats, which, though only collateral, is beautifully confirmatory of the general bearing of this law against the wave-theory of sound, to the consideration of which the reader's attention is especially invited.

I ascertained, by close calculation and measurement, that waves, while near the passing boat, or before they had traveled a sufficient distance to expend much of their force, *moved with considerably higher velocity than after they had reached to a greater distance.* But this proved to be entirely consistent with the principle evolved by the discovery of this fundamental law, as just explained, because the velocity of waves must necessarily decrease and their wave-lengths *contract* or shorten in the exact ratio as their amplitude becomes less!

There is no escape from this rule, as the reader no doubt already sees; for this *contraction of wave-length* and this *diminution of velocity* according to the ratio of *decrease in amplitude* is strictly and philosophically interdependent, and coincides with the laws of wave-motion, as here evolved. To elucidate the principle, it is plain to see if large waves travel faster than small ones, as my observations prove, then it follows that the front waves, as they spend themselves and diminish in amplitude, must necessarily lose in velocity, and, in so doing, will allow the waves in the rear, of larger amplitude, to constantly gain on those in front, thus shortening their distance from each other. In this manner the diminution in velocity naturally keeps pace with the diminution in amplitude, while the two combined mechanically result in this proportionate contraction or shortening of wave-length, exactly as my observations have shown to be the case.

If, therefore, there is the least analogy existing between actual wave-motion, as thus exemplified, and sonorous propagation, it must be perfectly clear to a logical mind that *a sound should travel slower and slower the farther it gets away from the generating instrument, while it should also become higher and higher in pitch by the contraction of its wave-lengths, as this is exactly the manner in which water-waves are propagated!* But since it is well known that sound retains the same *pitch* precisely, as well as the same *velocity*, however far its range may have extended from its source, as all observation proves, it becomes another and collateral demonstration that wave-motion is in no manner whatever connected with sonorous propagation, and that physicists are consequently laboring under a grievous philosophical misapprehension in their advocacy of the current theory of sound.

The law thus discovered—that all waves travel with a velocity exactly in proportion to their size and wave-length—not only serves the purpose of destroying the wave-theory of sound, but, while doing so, it beautifully accounts for certain phenomena which have been often observed but never explained, and which are, in fact, entirely inexplicable except by the key thus brought to light.

Take the well-known fact that every system of normal water-waves is accompanied by an occasional billow of very much larger proportions, which can be easily seen, at a considerable distance, looming up above its fellows. No doubt the reader has often observed this remarkable occurrence, and possibly wondered at the philosophical cause. I will now endeavor to explain the mystery, I hope satisfactorily, by applying this fundamental law of wave-motion just laid down.

As it is practically impossible for any two waves to be exactly of the same size, —as it is for any other two objects, large or small,—it is equally impossible for any two waves to travel with exactly the same velocity, since this law proves that their velocity must depend entirely upon their size. Hence, in the very nature of things, any wave which happens to be a small fraction larger than the one preceding it must necessarily gain slowly on the one *in advance, till at* last, overtaking it, the two blend into a single wave of about double the normal size of waves constituting that system.

The same thing then continues, after the two are united, with increased acceleration, requiring less time for this re-enforced billow to overtake the next wave in advance, owing to its increased velocity by such increase of size, till at last the accumulation results in these tremendous *king-waves*, as I shall call them, alone by the action of this elementary law of wave-motion, which thus again in another and unexpected way completely contravenes the wave-theory of sound, since no such disproportioned *sound-waves* are even claimed to occur in sonorous propagation by any writer on the subject! If sound consisted of wave-motion at all, or if air-waves were possible as the cause of sound-phenomena, we should certainly hear in every sustained musical tone an occasional outburst, or sonorous explosion, whenever one of these atmospheric *king-waves* should happen to accumulate and dash against the tympanic membrane! As no such sonorous effects are ever observed, it becomes clearly manifest that sound does not travel by means of air-waves at all, or by any principle analogous to undulatory motion.

Thus, aside from the philosophical value of a scientific explanation, never before attempted, of these natural phenomena of *king-waves*, it strengthens my general argument, based on this elementary law, by showing that every phase of true wave-motion is essentially subversive of the current theory of sound, since it is diametrically opposed to all observed sonorous phenomena. No rational man can doubt that, had Professor Helmholtz been aware of this law of wave-motion here demonstrated, namely, that wave-length and wave-velocity go hand in hand, he must have unconditionally abandoned the wave-theory of sound as a fallacy of science, and at once have sought some other hypothesis for solving the problems involved in sonorous propagation. As an honest physicist he could not have continued his adherence to a merely theoretical inference, after its very foundation had been swept away. In such an emergency, what could he have grasped as a basis of solution save the beautiful and consistent hypothesis of substantial sonorous pulses, which has been assumed and somewhat elaborated in the pages of this monograph, and which has never failed in rendering satisfactory explanations of all difficulties encountered.

In view of this law of wave-motion, which so completely destroys even the semblance of analogy between sonorous pulses and water-waves, Professor Helmholtz surely can not help seeing that fully one half of his great work on sound is thereby reduced absolutely to waste paper. One really can not help sympathizing with a writer under such circumstances. At least one half of this wonderful book, *The Sensations of Tone*,—a work which cost the author so many years of brain-struggle, and evincing a profundity of thought and mathematical formularization without a parallel in modern scientific research,—is based alone on the fundamental assumption, already quoted, that there is a complete similarity—an absolute parallel—between the action of sound-waves and water-waves, which, by the law thus demonstrated, is mercilessly scattered to the four winds. No reader can suppose, for a moment, that had this great investigator of science been aware of this law of wave-velocity, as so fully shown, that he could have repeatedly declared, as the fundamental principle of the wave-theory, that water-waves and atmospheric sound-waves are "essentially identical," "precisely similar," and travel "exactly in the same way." Evidently such language as this never could have found a place in his book, because it would have been devoid of the slightest foundation in truth, and hence so eminent and candid a savant as Professor Helmholtz could not have knowingly made these statements; and if the statements thus quoted could not be truthfully made, it is plain to see that the wave-theory, based upon them, can have no foundation in science or in the physical laws.

Starting out, however, with an honest mistake, originating in a pure fallacy of science, as the foundation of all his future reasoning on sound-propagation, he consistently built his elaborate castle in and upon the air, to be admired for a time by the physicists of the world as a beautiful and marvelous structure, but at last to fall into utter ruin at his feet by the fatal touch of a single philosophical fact! *

If there was, therefore, but this *one* conclusive argument against the wave-theory,—an argument, by the way, which the combined ingenuity of the world can neither jostle nor weaken,—Professor Huxley would say to physicists that their case was hopeless, and that they might as well abandon the wave-hypothesis at once. His words are big with meaning:—

"Every hypothesis *is bound to explain*, or at any rate *not to be inconsistent with, the whole of the facts it professes to account for;* and if there is *a single one of these facts* which can be shown to be inconsistent with (I do not merely mean inexplicable by,

* Since this argument was written, and mostly in type, Professor Robert Spice, to whom I have so often been indebted for valuable suggestions, has called my attention to the fact that the law here announced is admitted as correct in a recently published English work, though no details or measurements, as to the various proportions of wave length and velocity, are given.

but contrary to) the hypothesis, *such hypothesis falls to the ground—it is worth nothing. One fact* with which it is *positively inconsistent* is worth as much, and is as powerful in negativing the hypothesis, *as five hundred.*"—HUXLEY, *Lectures on the Origin of Species*, p. 140.

A truer and more concise rule of logic never was written. But if a *single* fact inconsistent with an hypothesis is sufficient to break it down, how irretrievably must the wave-theory have fallen to the ground when not a single fact or phenomenon in connection with the whole subject is found to be in its favor? On the contrary, every fact examined, and scores of others not touched upon in this monograph, point exactly in the opposite direction. It seems wholly inconceivable that such an array of pertinent considerations should conspire to break down the wave-theory, and yet that it, with all its absurdities and self-contradictions, should be the true solution of the sound-problem!

If these facts have really driven the wave-theory of sound to the wall, and demonstrated it to be a scientific fallacy, there is not a scientist who would not be willing to admit that the undulatory theories of *light* and *heat* are involved in the same catastrophe, and must share the same demolition, without striking them a blow, since it was only the sound-theory, as universally held, which led to the invention of *ether*, by which light as well as heat could be construed into some kind of undulatory mode of motion. As the wave-theory of sound—the very foundation of *ether* and *ethereal undulations*—has been shattered, it is clear to see that the superstructures reared upon it must necessarily fall to the ground.

In conclusion, I am well aware that to proclaim the overthrow of a universally accepted hypothesis, such as this of the undulatory theory of sound, which has stood the test of scientific investigation for hundreds of years, and which has never, so far as the writer knows, been called in question by a single physicist, or even for a moment doubted, has a presumptuous look on its very face,—amounting, it must be confessed, almost if not quite to audacity. But the facts, figures, and arguments, are here spread out, somewhat hurriedly, before the reader, while the appeal is now distinctly made to scientific thinkers and investigators either to show to the world that the considerations presented against the theory are erroneous or else to acknowledge their correctness, which I doubt not they will cheerfully do.

NOTE ON THE ANTENNÆ OF THE MOSQUITO.

Comments on the Hypothesis of Professor Mayer, as Published in the "American Journal of Science."

At pages 195, 196, &c., as the reader will recollect, I had occasion to examine the question of the unisonant vibration of the antennæ or so-called "auditory hairs" of certain invertebrates, such as those of the *mysis* or *opossum-shrimp;* and assumed, in opposition to Professor Helmholtz and other physicists, that any vibratory motion observed in such organs as the effect of sound must be regarded as simply *reactive* instead of *unisonant*, being first heard by the animal through the proper auditory organs, without any motion whatever of such parts, and then reflected back upon these antennæ or fibrillæ through the nervous system of the creature, thus causing the tremor which is noticed by experimenters as the supposed direct result of unisonant action.

This principle was illustrated by the reaction of sense-impressions causing subjective effects on different parts of the human organism, just as certain sounds, after being heard,—the *filing of a saw*, or some peculiar *scraping movement of a slate-pencil*, for instance,—will often react through the nervous system unpleasantly upon the *teeth*, and, with some temperaments, so set them on edge as to be almost unendurable. No one, of course, would suppose that such impression on the teeth could occur from the direct or objective action of sound-pulses, since a deaf person would perceive no such effect. This peculiar sensation can only be felt when the tone producing it has first passed to the brain through the proper auditory apparatus, and then reacted through another system of sense-nerves back upon the teeth. Such reactive connection between the teeth and the organs of audition is abundantly confirmed by the well-known experiment among dentists by which a violent toothache can be entirely checked for a number of seconds by pricking or pinching certain portions of the ear.

The truth is, no one, after a moment's reflection, will deny the correctness of the *reactive principle* here assumed as the most probable explanation of these tremulous movements of so-called "auditory hairs." To ignore the fact that certain external organs can be thrown into violent agitation as the effect of sound reacting through the nervous system, after it has been heard, would be to shut our eyes to the commonest experiences of human life. What reader has not seen nervous persons so startled by a sharp and unexpected sound that their hands would quiver and the whole frame tremble for some seconds after the shock? To attribute this *vibratory motion* of the hands and fingers to the direct or *unisonant* action of sound, as the reasoning of physicists would necessarily imply, instead of its *reactive* effect through the nerves after the auditory organs had performed their part of the work, would be to trifle with reason and stultify common sense, since, as before remarked, a deaf person, however nervous, would, of course, experience no such tremor of the fingers from sonorous shocks, however sharp.

While discussing this subject, in the fifth chapter, I gave what I still consider good and sufficient reasons for rejecting the possibility of such a thing as *microscopical fibrils* vibrating in *unison* to different sounds of the musical scale, since to be susceptible of such vibration (unless *forced* by very close contact), a string, rod, or fibril must itself be capable of producing that vibrational number, if plucked, which its length, weight, and rigidity, absolutely forbid.

Since those suggestions were in print I have read a carefully prepared article by Professor Mayer, in the *American Journal of Science*, for August, 1874, which had escaped my notice, in which he labors to show that the male *culex* or common *mosquito* hears sounds in the same way as described in the case of the *mysis*, by means of the variously tuned fibrils of his antennæ vibrating sympathetically to tones of various degrees of pitch, and that by this means he is enabled to hear the *female mosquito*, and thus direct his course toward her in the dark!

As this exposition of the auditory apparatus of the *culex*, given by Professor Mayer, involves the truth or falsity of the whole philosophy of audition and aural anatomy formulated by Professor Helmholtz as the basis of the wave-theory of sound, I propose to give a few moments to the considerations adduced in favor of such microscopical unisonant vibration.

I could entertain the reader with numerous interesting quotations from this ably written article, but will only make one or two brief extracts, to convey an idea of the general drift of the positions assumed. After experimenting with the antennæ of several mosquitoes, under the microscope, and observing the action of their fibrillæ while sounding a number of differently tuned forks, Professor Mayer remarks:—

"Experiments similar to those already given revealed a *fibril tuned to such perfect unison* with Ut_3 [one of Konig's tuning-forks] that it vibrated through 18 divisions of the micrometer, or ·15 mm., while its amplitude of vibration was only 3 divisions when Ut_4 was sounded. Other fibrils responded to other notes, so that I infer from my experiments on about a dozen mosquitoes, ***that their fibrils are tuned to sounds extending through the middle and next higher octave of the piano.***"

"*The song of the female* vibrates the fibrillæ of one of the antennæ more forcibly than those of the other. . . . The mosquito now turns his body in the direction of that antenna whose fibrils are most affected, and thus gives greater intensity to the vibrations of the fibrils of the other antenna. When he has thus brought the vibrations of the antennæ to equality of intensity, he has placed his body in the direction of the radiation of the sound, and he directs his flight accordingly; and, from my experiments, it would appear ***that he can thus guide himself to within*** 5° ***of the direction of the female.***"

It seems exceedingly strange, not to use a stronger word, that it never should have occurred to so careful an investigator of science to first *kill* the *mosquito* before making observations upon this supposed sympathetic vibration of its fibrillæ, as was suggested in the case of the shrimp, which could have been so easily done while the insect was secured under the microscope, by a little carbonic acid gas or by some other means, without marring the form of a single fibril! Instead of such *a practical* and fundamental thought occurring to this eminent physicist, he is particularly careful, in every instance, to inform the reader that he employed a "live" mosquito on which to experiment!

If his hypothesis of the ***unisonant vibration*** of a certain fibril through 18 divisions of the micrometer to the tone of Ut_3 is based on science, surely that particular fibril would have responded exactly the same after life was extinct, if not disturbed structurally, or else it did not vibrate unisonantly in the "live" insect! ***Any organ vibrating by sympathy to a Ut_3 fork does so because such unison body has a vibrational number corresponding to that of the exciting tone,*** which, of course, depends entirely upon its size, weight, and rigidity, and not upon the fact of the animal possessing such organ being either *alive* or *dead!* If Professor Mayer should find, on trying "about a dozen" of such lifeless mosquitoes with tuning-forks ranging through the entire register of the two octaves of the pianoforte, that not a single fibril could be made to stir,—as I predict, on general scientific principles, must be the case,—he would at once see that all this reasoning about the sympathetic vibration of microscopical organs was a fundamental philosophical mistake; and hence, that the supposed acoustical structure of the ear, including Corti's rods, as supporting the wave-theory of sound, must be simply visionary, having no correct basis in true science.

In such a contingency, there would be no conceivable explanation possible, as I doubt not Professor Mayer would admit, save the one given in Chapter V., already referred to, that all such tremors of the antennæ and fibrillæ of invertebrated animals, as the result of tone, is a reactive or subjective effect,—the tone reflecting, as it were, through the nerves of such animal organism back upon its external organs.

I thus venture the hypothesis, without trying a single experiment or knowing a thing about it except from my own reasoning, that the antennæ or fibrillæ of no *dead insect* or *crustacean* will ever respond sympathetically in the slightest degree to a tone when the vibrating body is a sufficient distance away *to prevent the incidental disturbance of the air from blowing them*, say, a distance of four or five feet.

Although the position here assumed is not necessarily vital to my argument against the wave-theory of sound,—that depending upon numerous direct considerations heretofore advanced,—I nevertheless give it a prominent place in the investigation of the collateral reasoning of physicists upon questions which are essential to the general correctness of their hypothesis; and I earnestly trust that these writers on sound will fairly test this question of the unisonant vibration of antennæ on dead insects, and if I am mistaken in my hypothetic reasoning on the subject, they are at full liberty, of course, to show me no mercy, as I surely do not deserve *quarter* when I refuse to give it.

It is a matter of astonishment, beyond words to express, as intimated when discussing Corti's arches, that physicists universally ignore this simple but fundamental acoustical law—that a rod secured at one end, in order to be capable of vibrating sympathetically in response to a tone of any determinate pitch, must, on being plucked, have the same vibrational number, or swing with the same normal periodicity, as the body producing the exciting tone; and that in order to thus correspond in vibrational number, its length, weight, and rigidity must at least approximately agree with those of the exciting instrument. Instead of taking this essential and elementary acoustical condition into the account, which, it would seem, ought to be the first thing a physicist would think of, Professor Mayer, following the example of Professor Helmholtz, assumes that a microscopical fibril on one of the antennæ of a mosquito may be "*tuned to such perfect unison*" as to respond to the middle A of the pianoforte, which, under the experience and skill of the best musical instrument makers, requires for its tone a string or rod at least *several hundred times longer than the fibril in question!*

This amazing absence of what I am compelled to call scientific perspicacity, in thus ignoring one of the most vital and fundamental principles of acoustics, seems to be but another illustration of what I have before referred to as the misguiding tendency of a false theory, even upon the greatest of intellects, when it once comes to be generally adopted as science.

If Professor Mayer really desires the world to place the least faith in his scientific "discoveries" that the microscopical fibrils of a mosquito's antennæ are actually "tuned to such perfect unison" with certain tones of the musical scale as to vibrate sympathetically when the corresponding tuning-forks are sounded, I insist that he shall experiment upon dead mosquitoes instead of "live" ones; and if he shall then fail to make a single "auditory hair" fall into unisonant vibration, I shall claim that *my* "discoveries" in regard to *nervous reaction*, "which I imagine are entirely new," have laid the true physiological and acoustical foundation for scientifically explaining the phenomena in question.

As a proof that the *tremulous action* of that particular fibril observed by Professor Mayer, under the microscope, and to which he specifically refers, was not *unisonant* but *reactive*, we have the fact, stated in his own words, as just quoted, that with one fork, Ut_3, it vibrated through 18, and with Ut_4 through 3 divisions of the micrometer;

whereas I now assert that a sounding body of any kind which would sympathetically vibrate in full unison to Ut_3, as did this fibril, would not respond at all to another fork as much out of unison as Ut_4 would be! This alone shows that the observed motion of this fibril was not the effect of unisonant or sympathetic vibration at all, but must be accounted for on some other hypothesis!

Of course, all this reasoning about the sympathetic vibration of these microscopical organs of insects, or the same kind of reasoning by Professor Helmholtz in regard to Corti's rods in his analytical investigation of the human ear, is simply intended to re-enforce the wave-theory of sound, and logically grows out of that general assumption. These far-fetched attempts, however, to show the periodic effects of air-waves on such microscopical organs are entirely unnecessary in order to account for the auditory powers of animals, either large or small.

It seems singular, to say the least, that a male mosquito in the dark is obliged to follow the direction indicated by the sympathetically vibrating fibrils of its antennæ in order to reach within *five degrees* of the singing female, when other animals, large and small, are capable of reaching their mates in a bee-line, in the darkest night, alone from listening to their cries, without the sympathetic vibration of any system of antennæ having fibrils tuned to two octaves of the pianoforte!

It is true Professor Mayer anticipates this objection, and attempts to meet it by assuming that other animals can turn their heads and shift their external ears so as to catch the direction of the sound by its varying intensity, as first one ear and then the other is employed; just as if a mosquito could not turn its head or its whole body, or shift its antennæ for that matter, in various directions, for the same purpose, —that is, supposing these antennæ to be really auditory organs which take the place of ordinary ears, which they may be, but which I neither affirm nor deny. Professor Helmholtz, in maintaining the unisonant vibration of such auditory hairs, claims their office to be the same in these lower animals as the Corti rods are in the higher species. But all this reasoning is forced, and falls vastly short of meeting this mosquito problem, since a hawk, by the sense of hearing alone, without external ears to shift, by simply turning its head or body to determine the proper line, can direct its course to within a good deal less than *five degrees* of the singing bobolink, as it often does this when its prey is completely hidden from sight by dense foliage. Yet C. Hasse, the eminent histologist and microscopist, assures us, as already quoted, *that the ears of birds are entirely destitute of Corti's rods!*

Thus, the "discoveries" of Professor Mayer, which he says "I imagine are entirely new," are proved to be "entirely" worthless, since a male mosquito ought to be able to hear the female and find his way to her in the dark without the unisonant vibration of its fibrils, if a hawk can perform as difficult a task without either antennæ or Corti's rods to vibrate sympathetically!

Instead of allowing the male mosquito to hear sound, in a common-sense way, by the direct action of the sonorous pulses falling upon his auditory organs, whatever they may be, and thus directly communicating the sensation, as to the direction of the sounding body, to the nerve-center, Professor Mayer complicates the whole process immensely, and more than triples the amount of geometrical calculation which this insect is obliged to make over ordinary animals before it can determine, after a sound-pulse strikes it, which way to steer! As proof of the correctness of this statement, see the last citation, in which this eminent authority assures us that the sound of the female first shakes, by sympathetic vibration, a properly tuned fibril on one of the male's antennæ which happens to be turned in the direction of such sound. The male culex, perceiving this sensation of the vibratory motion of that particular fibril, first locates it properly on this antenna, and then commences a course of geometrical calculation to ascertain which way to turn his body in order to allow the properly tuned fibril on the other antenna to receive a like sympathetic impulse. After this has been telegraphed to and from the nerve-center of the insect, the turning process commences, the mosquito in the mean time noting the gradual bringing into equal sympathetic play the properly tuned fibrils of both of the antennæ, and, by a difficult mechanical and mathematical course of reasoning, finally determines the exact point in the circle, "when he has brought the vibrations of the antennæ to *equality of intensity*"! When the two unison fibrillæ are thus made to vibrate with "equality of intensity," the fact is again communicated through this system of nerves to the seat of intelligence, where the operation is analyzed, and the decision then transmitted through another set of nerves to the muscles and ligaments of the wings, which finally put into execution the complete result of the routine of ganglionic processes, by which the insect is enabled to guide "*himself to within* 5° *of the direction of the female*"!

Now, if all this mechanical and geometrical ratiocination and acoustical analysis, and all this repeated telegraphing back and forth through different systems of nerves, must be gone through with by a male mosquito before he can determine within five degrees "the direction of the female," when a hawk can instantly fix the direction of an object it seeks by simply hearing its sound, without any unisonant vibration whatever, either of antennæ or Corti's rods, I am at a loss to see any practical or rational purpose in this almost infinitely more complex and ingeniously constructed organism of the culex, unless it be the work of an intelligent Creator, designed especially to convince physicists and naturalists of His existence!

Would it not be a much more reasonable assumption than this supposed sympathetic action of fibrillæ, though perhaps not so "entirely new," that one mosquito finds another in the dark by the sense of *smell*, on the same general principles by which it directs its course within the hundredth part of a degree toward the tip of a sleeping man's nose? If it could be shown by Professor Mayer that mosquitoes only annoy sleepers who *snore*, it might tend to corroborate his *unisonant* hypothesis! But the strict impartiality of such nocturnal visits, and the known capacity of the culex genus for finding almost any exposed square inch of a man's body, however dark the night, independently of any such directing unisonant capillary apparatus as sympathetically vibrating fibrillæ tuned to two octaves of the pianoforte, go strongly to demonstrate the inutility, to say the least, of any such a harp of a thousand strings in aiding this dipterous proboscidian to find his musical mate!

But if a mosquito determines the direction of a sound by the sympathetic vibration of certain fibrils on one or both of its antennæ, as Professor Mayer supposes, I would like to inquire of this high authority *how the insect knows when a particular fibril has been put into motion?* It surely does not *hear* it vibrate, for that would imply that it had an auditory apparatus independent of these fibrillæ sufficient for all practical purposes. It can not *see* such vibratory motion, for this is supposed to take place in the dark. Besides, if the male culex could see the motion of one of his own microscopical fibrils, he ought to be able to see the female! He must, therefore, depend alone upon the sense of *feeling* for a knowledge of this vibratory motion, whenever it occurs, as Professor Mayer would no doubt admit.

Now, to *hear* by *feeling* is about as anomalous an operation, and about as much a perversion of Nature's laws, as to *see* by *smelling*, or to *taste moonshine!* Aside, however, from this novel and absurd transformation and metamorphosis of the five senses, it is evident, if the motion of any particular fibril is *felt* by the mosquito, that such fibril must have a *tactile nerve* passing through it; and as there are several hundreds of these fibrils projecting from the antennæ of a single mosquito, it involves the enormous and extravagant waste of Nature's most precious materials in thus distributing hundreds of nerves belonging to one sense for the sole purpose of accomplishing the work of another! Why should Nature arrange four hundred tactile nerve-branches, extending through these fibrillæ, for the purpose of communicating to the ganglionic center of this insect the sensation of tone by feeling, when a single *auditory nerve*, properly brought to the surface of some part of the male mosquito's body, would have been amply sufficient to receive the substantial sonorous pulses of the female's music, as the corpuscular hypothesis so rationally supposes?

Such an operation as this is surely no more wonderful nor inconceivable than the analogous fact, which Professor Mayer can not ignore, that this same mosquito has evidently located on some part of its head or body an *olfactory nerve-membrane* which is capable of receiving the almost infinitely attenuated corpuscles of odor emanating from some other living animal, by which the sensation of *smell* is instantly transmitted to the seat of intelligence, and there translated not only into the knowledge of the proper *direction* of the odorous body, but is also resolved into such information as enables the insect to decide the *character* of the object smelt, whether it is favorable or unfavorable to its sanguiniverous appetite, *without any vibratory motion whatever!*

These two senses of *smell* and *hearing* are thus more than ordinarily analogous. I insist that, to a logical philosophical mind, the bare fact of imponderable and infinitesimal granules of odor, by simple contact with an olfactory nerve-membrane, being capable of conveying definite and complex intelligence to the brain of a living creature, *without any oscillatory motion of the air or of such nerve-membrane*, ought to be regarded as proof positive that acoustical impressions are made upon their appropriate nerve, and conveyed through it to the seat of intelligence in a similar way,—by the absolute contact of substantial sonorous corpuscles, without the aid of vibratory motion!

How it is possible for a thoughtful scientific investigator, after the subject has been brought to his attention, to believe, as he is obliged to do, in this manifest and acknowledged action of *odor*, and grasp the beautiful and consistent manner in which its impressions are received and analyzed, alone by *corpuscular contact*, and then instantly trample down all analogy and uniformity in Nature's laws by abandoning corpuscular action and resorting to *wave-motion*, requires more than human ingenuity to divine! It seems to the writer that this analogical consideration, when properly investigated and understood, ought to be alone sufficient to overthrow the wave-theory of sound, and at once to establish in its stead the *corpuscular hypothesis* as the only consistent solution of sound-phenomena, unless we admit that logic and reason have been banished from the earth.

Professor Tyndall refers approvingly to the course of reasoning by which an able physicist, in the time of Sir Isaac Newton, logically met and overthrew his emission hypothesis of light, and by which, as a strong analogical argument, the undulatory theory of light was aided if not finally established, till Newton himself was compelled to accept and advocate it. It was in this way: Let it be first understood that there was not a single scientist at that time who questioned the truth of the *wave-theory of sound*. Such a thought had never occurred to Newton or to any one else, so far as history records. Hence, the wave-theory of sonorous propagation was accepted, as a matter of course, as true science and as common ground upon which no dispute or even doubt existed. The argument, then, against the emission-theory of light was like this: Is it reasonable that *sound*, the first sensation above *odor*, should depart from the law of corpuscular contact and be produced by wave-motion, as all admit, and then that *light*, the next sensation above sound, should abruptly return to this same law of corpuscular contact which governs smell, rather than continue on as an undulatory motion of some sort of attenuated substance such as *ether* was assumed to be? On the basis of the wave-theory of sound being admitted as science, this logical mode of reasoning was simply irresistible. Newton and his coadjutors could not withstand it, and hence the *emission theory* of light fell to the ground, as it ought to have done with such scientific data as a foundation.

But think of the disaster which would have befallen his antagonists, had Newton been able to grasp the beautiful and harmonious consistency of Nature's laws, and to have hurled back upon their heads their own inevitable logic, re-enforced by the corpuscular hypothesis of sound? By simply appropriating their own argument, strengthened by a single modern improvement, he could have not only prevented the destruction of his emission-theory of light, but could have at once established the corpuscular theory of sound, thereby framing a consistent and uniform continuity in the nature and mode of operation of all the senses, from the lowest to the highest, as so fully illustrated at the close of the fifth chapter.

The time, however, had not yet come, and the age was not yet sufficiently ripe, for so radical and revolutionary a move as the overthrow of the universally accepted wave-theory of sound, and the establishment of the corpuscular hypothesis upon its ruins. I can not believe, from the arguments and considerations massed in this review, that it would be manifesting unjustifiable confidence in their unanswerable character, to assert that the time for such a scientific revolution has at last come; and that, could the great Newton be permitted to look down from his higher sphere upon the progressive strides scientific investigations are making, and behold the tables turned upon the logic which trailed the banner of his emission-theory in the dust, he would now have his revenge.

☞ See Note on *Telephone and Phonograph*, page 523.

NOTE ON THE TELEPHONE AND PHONOGRAPH.

When the revised chapters on *Sound* were being written, the telephone was but just coming into general notice, and, of course, was but partially understood by any except those who had made it a special study. Up to the completion of the volume I had not had an opportunity of carefully examining this remarkable invention. Since the revision of the work, I have had frequent inquiries from friends at a distance as to whether the telephone does not contravene the corpuscular hypothesis of sound, as assumed in this treatise, and go to favor the wave-theory, as held by all physicists. To meet these inquiries, I have carefully investigated the instrument,—one of Professor Bell's improved telephones, which was kindly furnished me by Mr. W. K. Applebaugh, General Superintendent of the Telephone Company of New York, 203 Broadway,—the result of which I now lay before the reader.

It will not be necessary here to enter into a detailed description of the instrument or its construction, as this is so well understood, having been repeatedly explained by various writers in a number of different scientific publications. A brief general description, however, may be necessary, in order to properly convey my ideas concerning its relation to the wave-theory of sound.

The instrument consists of a magnetized steel bar, about three eighths of an inch in diameter and five inches long, wound at one end with fine insulated copper wire, and a circular membrane of soft iron about two inches in diameter and the thickness of common writing-paper. This membrane is secured and held by its rim in the frame of the instrument in such a manner as to leave its center free to vibrate by the least possible movement of the air against its surface. The frame also supports a concave mouth-piece, with an opening in its center for the purpose of converging the atmospheric disturbance upon the center of the membrane when talking to the instrument.

Now, it is a fact in science, but one which we can not explain, that when the ends of the wire, coiled around a permanent steel magnet, are joined together, a current of electricity is generated by the magnetism; and it is also a fact, just as inexplicable, that if a piece of soft iron is brought alternately near to and away from the end of such bar, it affects the electric current passing through the wire by making it alternately weaker and stronger. These two facts constitute the foundation of the Bell telephone.

It will now be readily understood, if the membrane of soft iron should be secured in the frame close to the end of the magnetized bar but not near enough to touch it, that whenever such membrane is stirred in the slightest degree, either by the motion of the air or by any other force, it must correspondingly affect the electric current of the wire and the strength of the magnet around which it is coiled; and hence, if, in talking to the membrane through the mouth-piece, we cause a disturbance of the air which vibrates it, moving it alternately toward and from the magnet, it is plain that each motion to and fro, however small, is represented by a corresponding weakening or strengthening of the electric current, and consequently of the attractive power of the magnet.

We have then only to suppose this wire, before its ends are joined, to be coiled around another steel bar in another telephone, exactly the same (however distant the two instruments may be apart), and it is evident that the distant magnet will be acted on by the electric current, and alternately weakened and strengthened in all respects the same as the first one, and will in turn act on its membrane through the law of attraction, giving it exactly the same motions of the transmitting membrane whose vibrations manipulate the varying force of the electric current. The motions produced by the air-waves in the first or transmitting membrane being exactly reproduced by the magnetic bar in the second or receiving membrane, it is plain that these latter motions will necessarily generate exactly the same tone which first gave the motion to the former membrane, through the aerial waves driven against it by the spoken words.

So far there is no controversy in regard to either the motions or their effects, and the phenomena here explained prove nothing one way or the other as to the truth or falsity of the wave-theory, as I will briefly endeavor to show. Throughout the *Evolution of Sound* it has been repeatedly shown that the vibrations of any sounding body—the human vocal apparatus as well as other instruments—cause a corresponding succession of air-waves to pass off for a limited distance around, but that these air-waves are only an incidental effect of the motion generating the sound, and not, by any means, the sound itself. For a circumscribed distance around the sounding body, the waves—passing off with exactly the force and rapidity of the accompanying sound-discharges—will, of course, by impinging upon a sensitive membrane, throw it into forced vibration, in exact conformity to the original vibration which generated the tone and the accompanying wave-motions which are thus sent off. Such forced tremor occurs whether the membrane is in unison with the sounding body or not, but can not occur outside of the limited distance traversed by such incidental air-waves, unless in unison.

In this way it was shown that the tympanic membrane might be caused to vibrate by loud words spoken into the ear, or close by it. In the same way, as I pointed out in the fifth chapter, the whole external ear, as well as the fingers, might be thrown into vibratory motion by the air-waves of a steam-whistle in close proximity; but such forced tremor, though corresponding in rapidity to the vibrational number of the sounding body, is but an incidental or coerced effect of the air-disturbance caused by the same vibratory motion which generates the tone, though it is as different from the sound-pulse itself as is the compressed air-wave which destroys buildings at a magazine explosion different from the accompanying sound-pulse, as so elaborately illustrated at page 103, and onward.

As a proof that sound and air-waves are two separate and distinct phenomena, it is evident that if the membrane of the telephone could be moved back and forth by any direct mechanical means other than air-waves, such as a delicate system of levers, acting on it with all the variety of *rapidity*, varying *amplitude*, and *force* which governs its motion when certain words are spoken into the mouth-piece, it would produce precisely the same result on the varying intensity of the electric current and strength of the magnet, and consequently would reproduce the same variety of movement in the membrane of the telephone at the other end of the line, causing the words to be repeated there, alone by mechanical means, the same as if they had been originally spoken into the mouth-piece by means of the vocal organs.

As a proof of this, we have only to look at Mr. Edison's astonishing invention of the *Phonograph*, which actually accomplishes the equivalent of what I have here described, but without levers. By properly attaching a steel point to the center of a telephone membrane, so adjusted as to press into the delicate spiral groove of a revolving cylinder enwrapped with tin-foil, the vibration of the membrane, acted on by the waves accompanying spoken words, is made to record a corresponding variety of impressions on the foil, in the form of delicate indentations of varying depths and undulatory lengths. Then, by re-revolving the cylinder in the same direction with the point in the same line of indentations, the membrane is, of course, forced through the same variety of movements which produced the record, and is thus made to re-generate the original words exactly the same as when spoken;

and this same thing can, of course, be done any number of times, by retaining the foil record, and at any future dates desired, thus preserving the speech of friends, as we now preserve their photographs, for future generations.

I have not the least doubt but that the wonderful mechanical genius of an Edison, a Gray, or a Bell, can, and possibly will, yet produce a purely mechanical means of operating on such a membrane through some kind of keyboard and levers, by which a deaf and dumb person may learn to talk in oral words by the manipulation of keys, the same as he might learn to play a tune on the piano without being able to hear it. I shall not be at all astonished if such a device should be announced as the next wonderful production of one of these prolific inventors. For surely if a greatly magnified longitudinal sectional view of the line of indentations made by a phonograph-point could be taken, it would form the basis of such mechanical movements as would lead to proper devices for a true talking-machine, and which would produce exactly the varied motions in a membrane necessary to the generation of spoken words.

The marvelous thing about the telephone, however,—the wonder of wonders,—is, that the electric current passing through the wire connecting the two instruments can be caused to vary in quantity and force so sensitively, and with that almost infinite nicety, by the mere tremor of the transmitting membrane, as to reproduce this exact vibratory movement in the receiving membrane, and thus re-generate the same tone! No one, with any degree of scientific knowledge, will for a moment suppose that the sound passes through the whole length of the wire from one instrument to the other when hundreds of miles apart, or that any motion corresponding to the vibration of the transmitting membrane can take place in the wire, save that of the varying quantity and strength of the electric fluid. No conceivable or possible tremor can be supposed to take place in the copper wire itself, nor is it necessary for any such motion, any more than that the supposed luminiferous ether-waves passing through a diamond should displace and undulate the texture of that adamantine substance. Then, if the wire remains quiescent in the passage of the electric fluid, there is no wave-motion at all taking place between the two instruments; but the substantial current of electricity takes up the substantial sound-pulses of the vocal organs, and by reproducing the original motions at the other end of the line, re-generates the original tones.

I submit, then, that the fact of a sound-movement, such as that of spoken words, producing a limited and incidental effect upon the surrounding air in the form of waves, and thus causing a corresponding motion in a sensitive membrane such as that employed in the telephone, is in perfect accordance with the corpuscular hypothesis of sound, as maintained in this work; and does not, in the remotest degree, go to favor the monstrous but unavoidable assumption of the wave-theory that a mere insect, by the motion of its legs, exerts a mechanical force upon four cubic miles of air sufficient to oscillate *two thousand million tons* of tympanic membranes, if they should happen to be present, as it must absolutely do if the wave-theory has any foundation in science. (See page 175, and onward.)

But it is urged that, in the body of this work, I have under-estimated the distance around a sounding body to which its air-waves will travel, as shown by recent experiments with the improved carbon telephones of Mr. Edison and Prof. Hughes, since words spoken a hundred or more feet away from the transmitting device have been conveyed through the electric wire and reproduced at the receiver. It does not follow, however, that this is done by air-waves, or that no effect can be produced by sound itself acting on the electric current of these telephones, except through the mechanical vibrations of the membrane or other transmitting device, as will soon appear. But even granting that I have under-estimated this distance, and that these air-waves may really travel hundreds of feet from the sounding body, is it not still far more reasonable to suppose that such waves are but an incidental disturbance of the air instead of constituting the sound itself, rather than to assume, as the wave-theory compels us to do, that an insect is actually capable of exerting the inconceivable mechanical force just intimated? One or the other of these views is clearly unavoidable.

There is absolutely no imaginable limit to the *tenuity* of substance, as witness the so-called luminiferous *ether*, *while there is a clearly and abruptly defined limit to the exertion of mechanical force, determined solely by the physical strength of the being which acts*. (See argument on the tenuity of odor, page 134.) Hence, while we positively know that an insect can not displace or stir a single *ton* of ponderable matter (to say nothing of shaking *two thousand million tons*, as required by the wave-theory), we confessedly do not know but that a locust might surcharge a hundred cubic miles of air with some kind of substantial pulses without appreciably reducing its own weight, since the tenuity of substance, as all science admits, is without conceivable limit. Does not this, of itself, point with infallible certainty to the corpuscular hypothesis of sound, rather than to the infinitely impracticable assumption of wave-motion? And does it not, therefore, devolve upon physicists to seek some other explanation of these telephonic mysteries, instead of trying to force them into the service of a theory involving such stupendous impossibilities as those just alluded to?

From the marvelously delicate effect developed by Prof. Hughes' instrument, *by which the step of a fly can be heard through the electric wire miles away*, it seems highly probable, if not absolutely certain, that the physical oscillations of the transmitting device have nothing to do with it, but that, instead of air-waves sent off from a fly's foot producing such a result, the substantial sound-pulse itself, thus generated, acts directly upon the *electric fluid* of the wire through the carbon or other materials of the transmitter. I strongly suspected, and even urged when writing this work, that the so-called correlation of forces would turn out to be a correlation of substantial emanations; and thus that sound-pulses, light-emissions, heat-radiations, electric currents, &c., would be found to sustain such a mutual relation to, or affinity for, each other, that by mingling in certain ways they could act upon and modify the effects of each other.

Many experiments have shown that light, as well as heat, affects the electric condition of bodies,—electricity in turn being convertible into light, heat, and sound; while heat is well known to act directly on sonorous pulses, rapidly increasing their velocity up to a certain degree, and then decreasing it. What, then, should hinder the effect here intimated, of substantial sound-pulses acting upon the substantial electric current, aided by suitable mechanical or chemical appliances?

These sensitive telephonic effects would seem fully to corroborate such a substantial correlation, rather than go to support the view that air-waves, sent off by the movement of a fly's foot, *could, by any possibility, exert sufficient physical force to alternately compress and expand a solid glass tube or a stick of carbon*, and in this manner alternately strengthen and weaken the electric current passing through the wire! As well might we expect to alternately compress and expand Chimborazo, by whistling at it a mile away from its rocky base.

Everything tends to favor the opinion now being formed by able scientific thinkers that something more than mechanical air-waves is necessary to produce the infinitely delicate effects generated at the transmitting device of a carbon telephone, or *microphone*, as it is sometimes called. No thought so readily and rationally comes to our aid as the corpuscular hypothesis of sound, in connection with this law of correlation and the interconvertibility of the so-called forces of Nature,—thus teaching us that the substantial sound-pulse itself, impinging upon the substance of the electric fluid through the sensitive unhomogeneous substances of these telephones, generates a tremor in the electric current corresponding to its own vibrational number.

Some of our greatest physical investigators do not hesitate to claim that even the more delicate telephonic effects produced through the Bell diaphragm can not be attributed to its mechanical or bodily vibrations toward and from the pole of the magnetized bar. The eminent Scotch physicist, R. M. Ferguson, Ph.D., F.R.S.E., distinctly takes this position in a lecture on the telephone recently delivered before the Royal Scottish Society of Arts, as copied into the *Scientific American Supplement*, No. 120.

Dr. Ferguson shows, by the most convincing arguments, that the mechanical oscillation of this iron disk is wholly insufficient to account for some of the effects produced in the transmission of articulate speech; though he admits that these bodily movements of the membrane produced by air-waves from a sounding body in close proximity add to the loudness and distinctness of the message. As a proof that but a portion of these effects can come from the vibratory motion of the transmitting membrane, he notes the fact that a solid iron plate, *an inch thick*, in place of the membrane, has produced distinct transmissions of speech, and that even the naked end of the magnetized bar has done the same thing without the intervention of any kind of diaphragm or plate. What clearer evidence could be asked in favor of the position just assumed of an actual correlation existing between the substantial sound-pulse itself and the electric fluid of the magnet? In speaking of the common explanation of the telephone, as given by all writers on the subject,—that is, that the transmission of speech depends entirely upon the mechanical vibration of the transmitting membrane,—the Doctor remarks:—

"This explanation is beautiful and simple, and one would wish it true; it must always remain the popular one. Undoubtedly, however, when narrowly examined it is found to be a mere hypothesis, and to have as yet no experimental confirmation. . . . I would, in the first place, take exception to the vibratory theory of Bell, viz., that it is the vibrations of the disk to and from the pole of the magnet, *in excursions proportionate to the intensity, pitch, and quality of the vocal sounds*, that electrically affect the instrument; and in so doing I only express the dissatisfaction with it of *almost every one* who deals with the telephone. *The mere vibrations of the iron disk are insufficient to account for its action.*"

If, then, the mechanical motions to and fro of the membrane fail to account for these telephonic effects, I submit that the mechanical air-waves which cause such vibrations must also fail. Is it logical or reasonable to reject the vibratory motion of the sending membrane as a sufficient cause of transmitted speech through an electric wire, and still cling to the wave-theory, on which alone such defective explanation of the telephone depends? Yet, strange as it seems, after Dr. Ferguson had made such an important advance, discarding the possibility of mechanical vibrations as a sufficient cause of these telephonic effects, he still persists in holding to the wave-theory, and is unable to take the one short remaining step which would have led him directly to the corpuscular hypothesis, and thus have completely solved the problems he was discussing! Instead of this simple mode of cutting the gordian knot, he submits an explanation immeasurably more difficult to accept than the one he controverts, namely, that the mechanical air-waves sent off from a sounding body, even though they are too feeble to cause the least vibratory motion of a thin membrane, are nevertheless powerful enough to act upon the tissue and fiber of the magnet, driving its metallic molecules into undulations, thus literally displacing the atoms of the steel bar itself. These old-fashioned, theoretic air-waves, it seems, according to this high authority, are not capable, especially when weak, of stirring this membrane mechanically, as he clearly demonstrates by substituting an iron plate an inch thick, yet they are strong enough to churn its material particles into condensations and rarefactions! He holds that the *molecules*, which are simply the smallest particles of the iron, are actually displaced, and caused to change in their relative position to each other by the action of "external sounds," *and that this sonorous contact generates currents of electricity.* Speaking of the office of the Bell membrane, he says:—

"It is an acoustic instrument *sui generis*, and its smallness seems to point to *molecular* as well as *vibratory* action. . . . Sound acts on iron so as to produce molecular changes, the electric power of which is much enhanced by *the vibration of the sounding body*. . . . I have endeavored to prove that in future books of science Bell's discoveries will be given as twofold: first, having devised perhaps the best way of developing magnetic sounds in iron; and second, of *showing that the condition produced in iron by external sounds results in electricity.*"

Now, if "external sounds" can actually produce *electricity* in a steel bar or in the iron disk of a telephone as well as vibratory motion, it is plain that sound must be something more than mechanical air-waves. This view is fully confirmed by Mr. Edison himself. He says:—

"I discovered that my principle [the alternate compression and expansion of carbon by sound-waves], unlike all other acoustical devices for the transmission of speech, *did not require any vibration of the diaphragm.* That, in fact, sound-waves *could be transformed into electrical pulses without the movement of any intervening mechanism.*"—PRESCOTT'S *work on the Telephone*, p. 226.

Thus the inventor of the carbon telephone and the phonograph supports the law of correlation and interconvertibility as here urged, and in doing it he overthrows his own assumption of the alternate compression and expansion of a lump of carbon by the action of air-waves, since that would evidently be an "intervening mechanism," as much so as the alternate elongation and contraction of an india-rubber cord, or the vibratory movement of a Bell membrane. This position is also fatal to the wave-theory of sound, as it is clear that simple aerial undulations can not be "transformed into electrical pulsations" or into any thing else except air in a quiescent state. Hence, sound must be something more than air-waves.

I submit, therefore, to the reader if the tendency of scientific investigation is not in a direct line toward the corpuscular hypothesis, for the first time formularized, or even hinted, in this monograph; and if the researches and distinct announcements of these eminent authorities, as just quoted, which so clearly show that something besides mechanical vibration or "the movement of any intervening mechanism" is necessary to account for telephonic effects, do not as clearly dispense with the wave-theory of sound.